UNIVERSITY OF MICHIGAN
OCT 26 1954
PERIODICAL READING ROOM

1831 1954

SED · OMNIA · DISPOSVISTI

YORK · OXFORD · CAMBRIDGE · EDINBURGH · DUBLIN · BRISTOL · LIVERPOOL · NEWCASTLE · BIRMINGHAM · GLASGOW · PLYMOUTH · MANCHESTER · CORK · SOUTHAMPTON · SWANSEA · IPSWICH · BELFAST · HULL · CHELTENHAM · LEEDS

BATH · NOTTINGHAM · DUNDEE · NORWICH · EXETER · BRIGHTON · BRADFORD · SHEFFIELD · SOUTHPORT · MONTREAL · CARDIFF · TORONTO · DOVER · S. AFRICA · WINNIPEG · LEICESTER · PORTSMOUTH · AUSTRALIA · BOURNEMOUTH · LONDON

ABERDEEN ☆ ☆ ☆ BLACKPOOL

The ADVANCEMENT OF SCIENCE

Vol. XI, No. 42

SCIENCE AND HUMAN NATURE

Presidential Address by

Dr. E. D. ADRIAN, O.M., P.R.S.

and

Addresses by

PRESIDENTS OF SECTIONS

Oxford : 1954

Published by the

BRITISH ASSOCIATION

for the Advancement of Science

Burlington House, London, W.1.

September 1954 Tel.: Regent 2109

Price : Seven Shillings and Sixpence

PITMAN

A SELECTION FROM THE TECHNICAL LIST

The Metallurgy of Welding

By Walter H. Bruckner, B.A., B.S., Ch.E. The specification of metals for welding, the design of a welded joint, the compounding of a coating for a metallic electrode used in welding, and a host of other considerations, can be better understood with the help of the background supplied in this book. Illustrated. 30/- *net.*

Telecommunications

By A. T. Starr, M.A., Ph.D., M.I.E.E. This new book is written to cover the syllabus for the subject of Telecommunications in the London University Degree. The mathematical methods of both Fourier Analysis and Operational Calculus have been included, and a short account is given of microwave and waveguide theory which will serve as an ideal introduction to advanced work. 454 pages. 35/- *net.*

The Practice of Local Anaesthesia

By George Bankoff, M.D., D.Ch., F.R.F.P.S., F.R.C.S.E. This is the fourth edition—the first published by Pitman—of a book which provides a practical and concise survey of the whole field of modern local anaesthesia. Illustrated. Ready soon. Approx. 25/- *net.*

Recent Developments in Psychosomatic Medicine

Edited by Eric D. Wittkower, Associate Professor of Psychiatry, McGill University, and R. A. Cleghorn, Associate Professor of Psychiatry, McGill University. This book brings together the contributions of 24 experts in the field of psychosomatic medicine and forms a comprehensive account of various aspects of the subject. 50/- *net.*

Pneumoconiosis Abstracts Vol. II (1939–1950)

Reprinted from "The Bulletin of Hygiene." Contains all the abstracts of papers referring to pneumoconiosis published in The Bulletin of Hygiene from 1939–1950. 80/- *net.* Vol. I (1926–1938), 63/- *net.*

Atomic Energy and its Applications

By J. M. A. Lenihan, M.Sc., Ph.D., F.Inst.P., Physicist to the Western Regional Hospital Board, Scotland. This book is neither a textbook of atomic physics nor a catalogue of its applications, but combines the most valuable features of both. It provides a simple guide to the physical foundations of the subject and a systematic exploration of its practical possibilities. 22/6 *net.*

DETAILS OF OTHER BOOKS FROM:

SIR ISAAC PITMAN & SONS LTD.

PARKER STREET · KINGSWAY · LONDON · W.C.2

ADLARD & SON LTD.

BARTHOLOMEW PRESS, DORKING, SURREY

Telephone: Dorking 3874

PRINTERS TO SCIENTIFIC SOCIETIES

Experienced in mathematical formulae composition

CIMBRA

(Comércio Império Britânico-Brasil)

IMPORTERS OF

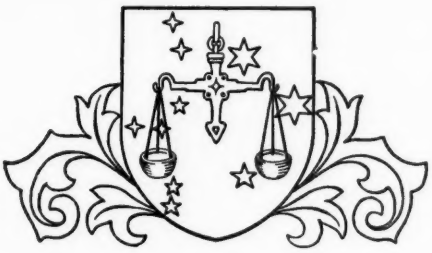
ROCK CRYSTAL QUARTZ

FOR PIEZO ELECTRIC AND OPTICAL PURPOSES

We are at your service PLEASE CONSULT US

CIMBRA LIMITED

123 PALL MALL, LONDON, S.W.1 WHITEHALL 9661 CIMBRALON, PICCY, LONDON

1831  1954

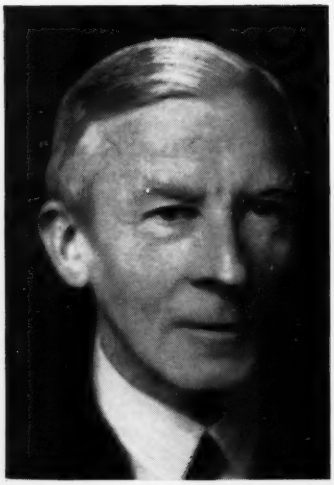
THE ADVANCEMENT OF SCIENCE

VOL. XI, No. 42 SEPTEMBER, 1954

CONTENTS

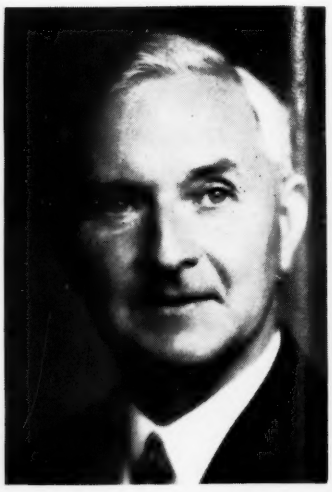
Addresses delivered at the Annual Meeting of the British Association for the Advancement of Science in Oxford, September 1 to 8, 1954.

	PAGE
PORTRAITS OF THE PRESIDENT OF THE ASSOCIATION AND OF PRESIDENTS OF THE SECTIONS	118
OFFICERS AND COUNCIL	120
SCIENCE AND HUMAN NATURE, *by* DR. E. D. ADRIAN, O.M., P.R.S.	121
RECENT DEVELOPMENTS IN NUCLEAR PHYSICS, *by* SIR JOHN COCKCROFT, K.C.B., C.B.E., F.R.S.	129
NEW IDEAS IN CHEMISTRY, *by* SIR JOHN LENNARD-JONES, K.B.E., F.R.S.	136
THE RELATIONSHIP OF PALAEONTOLOGY TO STRATIGRAPHY, *by* DR. C. J. STUBBLEFIELD, F.R.S.	149
ARCHAEOPTERYX AND EVOLUTION, *by* SIR GAVIN R. DE BEER, F.R.S.	160
THE COAST AND THE GEOGRAPHER, *by* PROF. J. A. STEERS	171
THE CHANGING STRUCTURE OF THE BRITISH ECONOMY, *by* PROF. E. A. G. ROBINSON	182
THE EFFECT OF NEW MATERIALS ON ENGINEERING PROGRESS, *by* DR. WILLIS JACKSON, F.R.S.	194
COLONIAL ARCHAEOLOGY, *by* SIR MORTIMER WHEELER, C.I.E.	203
THE RELIEF OF PAIN, *by* PROF. A. D. MACDONALD	212
RECENT STUDIES IN THE PSYCHOLOGY OF THINKING, *by* PROF. L. S. HEARNSHAW	220
GROWTH AND PRODUCTION, *by* PROF. W. H. PEARSALL, F.R.S.	232
THE TEACHER IN THE TWENTIETH CENTURY, *by* MR. RONALD GOULD	242
AGRICULTURE : THE INDUSTRY AND THE SCIENCE, *by* DR. R. E. SLADE	251



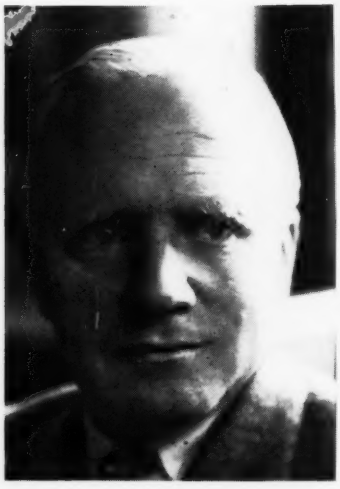
Dr. E. D. Adrian
1.

Sir John Cockcroft
2.



Sir John Lennard-Jones
3.

Dr. C. J. Stubblefield
4.

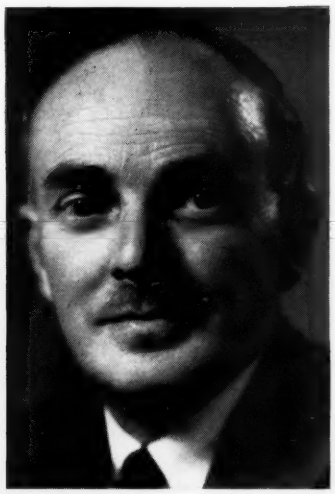


Sir Gavin R. de Beer
5.

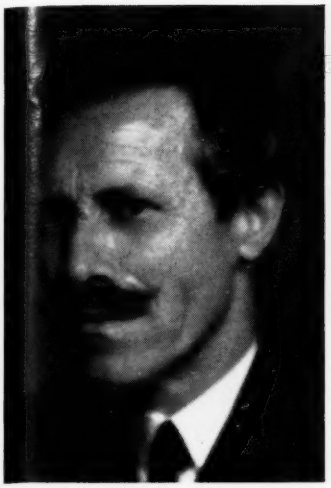
Prof. J. A. Steers
6.

Prof. E. A. G. Robinson
7.

Portraits 1, 2, 3, 9, 12 by Walter Stoneman, London.



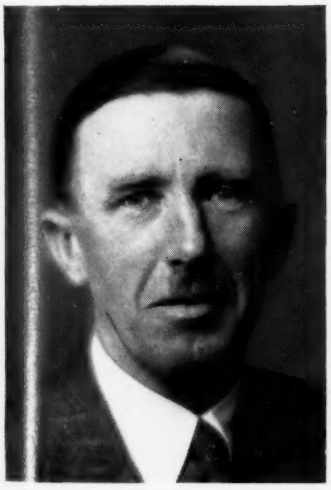
Dr. Willis Jackson
8.



Sir Mortimer Wheeler
9.

Prof. A. D. Macdonald
10.

Prof. L. S. Hearnshaw
11.



Prof. W. H. Pearsall
12.

Mr. Ronald Gould
13.

Dr. R. E. Slade
14.

Portraits 4, 5, 8, 13 by Elliott & Fry Ltd. London. Portrait 14 by Ramsey and Muspratt, Cambridge.

OFFICERS AND COUNCIL OF THE BRITISH ASSOCIATION, 1954-5

PATRON.

HER MAJESTY THE QUEEN.

PRESIDENT, 1954.

Dr. E. D. Adrian, O.M., P.R.S.

GENERAL TREASURER.

M. G. Bennett.

GENERAL SECRETARIES.

Sir Richard Southwell, F.R.S. | Dr. George Taylor.

ORDINARY MEMBERS OF THE COUNCIL.

R. J. Bartlett.
Prof. L. F. Bates, F.R.S.
M. C. Burkitt.
Prof. H. G. Champion, C.I.E.
Sir Alfred Egerton, F.R.S.
Prof. H. P. Gilding.
Dr. Ezer Griffiths, O.B.E., F.R.S.
Dr. D. B. Harden.
Prof. H. R. Hewer.
Prof. K. S. Isles.
Dr. Willis Jackson, F.R.S.
A. Gray Jones.
Sir Harold Spencer Jones, F.R.S.
Prof. H. D. Kay, C.B.E., F.R.S.
Sir Ben Lockspeiser, K.C.B., F.R.S.
Prof. Kathleen Lonsdale, F.R.S.
Prof. A. D. Macdonald.
J. Wickham Murray, O.B.E.
Prof. F. W. Paish.
Prof. W. J. Pugh, O.B.E., F.R.S.
Dr. J. Ramsbottom, O.B.E.
Lord Rennell of Rodd, K.B.E., C.B.
Prof. L. Dudley Stamp, C.B.E.
Dr. W. E. Swinton.
Dr. C. Tierney.
Prof. P. E. Vernon.
Prof. W. Wardlaw, C.B.E.
W. H. Wilcockson.
Dr. H. E. Wimperis, C.B., C.B.E.
Dr. N. C. Wright.

EX-OFFICIO MEMBERS OF THE COUNCIL.

Past-Presidents of the Association, the President for the year, the President and Vice-Presidents for the ensuing Annual Meeting, past and present General Treasurers and General Secretaries, and the Local Treasurers and Local Secretaries for the Annual Meetings immediately past and ensuing.

PAST PRESIDENTS OF THE ASSOCIATION.

H.R.H. The Duke of Windsor, K.G., F.R.S. (1926).
Prof. Sir Arthur Keith, F.R.S. (1927).
Sir Henry Dale, O.M., G.B.E., F.R.S. (1947).
Sir Henry Tizard, G.C.B., F.R.S. (1948).
Sir John Russell, O.B.E., F.R.S. (1949).
Sir Harold Hartley, K.C.V.O., C.B.E., M.C., F.R.S. (1950).
H.R.H. The Duke of Edinburgh, K.G., K.T., F.R.S. (1951).
Prof. A. V. Hill, C.H., O.B.E., F.R.S. (1952).
Sir Edward Appleton, G.B.E., K.C.B., F.R.S. (1953).

PAST GENERAL OFFICERS OF THE ASSOCIATION.

Prof. P. G. H. Boswell, O.B.E., F.R.S.
Prof. Sir David Brunt, Sec. R.S.
Sir Harold Hartley, K.C.V.O., C.B.E., M.C., F.R.S.
Dr. Edward Hindle, F.R.S.
Sir John Lennard-Jones, K.B.E., F.R.S.
Sir Frank Smith, G.C.B., G.B.E., F.R.S.
Prof. F. J. M. Stratton, O.B.E., F.R.S.
Sir Arthur Trueman, K.B.E., F.R.S.

HON. AUDITORS.

Sir John Simonsen, F.R.S. | Dr. H. J. T. Ellingham.

SECRETARY.

Sir George V. Allen, C.B.E.

SCIENCE AND HUMAN NATURE

Address by

DR. E. D. ADRIAN, O.M., P.R.S.

PRESIDENT OF THE ASSOCIATION

THREE hundred years ago a scientific society used to meet in Oxford every week in the rooms of Dr. Wilkins of Wadham College. It was the first of its kind in our country and it had what we should now call an escapist motive. Fifty years before Francis Bacon had published his great plan for a new road to knowledge, by concerted observation and experiment, and Harvey soon after had made the experiments which proved the circulation of the blood. But then the country had been torn by civil war. It had killed its King and could not see how to reach settled government. Dr. Wilkins had collected a band of scholars of inquiring mind, Boyle, Wren, Willis and others whose names are now part of the history of science, and they met together to take what comfort they could in the new kind of knowledge which could be confirmed by experiment instead of the appeal to authority. 'Their first purpose was no more than only the satisfaction of breathing a freer air and of conversing in quiet with one another without being engaged in the passions and madness of that dismal age.'

So it was here that the scientific age was conceived in England, with the conflicting loyalties of the Commonwealth as a background, and I can admit Oxford's claim the more cheerfully because Dr. Wilkins moved to Cambridge and was for a short time Master of Trinity College, just before Isaac Newton came there as a young undergraduate.

When the King came to his own again the temper of opinion changed. There were great prospects ahead. The philosophers moved to London convinced that their inquiries would lead to material prosperity as well as to deeper knowledge. They founded The Royal Society and Isaac Newton's *Principia* established the mechanical order of the world. The material progress followed more slowly, but one hundred years after Newton's death it had already begun to affect the lives of half the people in this country. Scientists as usual were in no doubt about the value of what they were doing and they felt it was high time to share their faith and some of their responsibilities.

In 1831, therefore, the British Association was founded for the Advancement of Science and at the annual meeting our leading scientists assembled in one big town after another to spread the news of this fresh source of knowledge and of material advance; and the Association has gone round ever since on much the same errand, announcing new discoveries and airing controversies before an audience which has grown steadily in size and in its understanding of what the scientists are about.

But now the products of science are everywhere. Knowledge of the material world is constantly growing and its consequences are of such a spectacular nature that everyone is aware of them: few people now can doubt that the scientist's picture of the world must have some validity if it enables him to deliver such remarkable goods. So it has come about that the advancement of science is in everyone's mind. Why then are we here to-day? What is there left for the British Association to do if it can only preach to the converted?

For scientists it can do a great deal. We are all specialists nowadays and here we can learn what is happening in fields outside our own, but it is true that these meetings must lack some of the appeal they had when the whole territory of science was so much smaller. Progress is too rapid for the new discoveries to be

saved up as a birthday present and controversies have become too technical to be aired in the market place. Yet there is one very great and worthy task which needs the help of a body like ours which brings scientists and laymen together. The Association must show the layman where the scientific age is leading him. It is by its impact on public opinion that the success of these meetings must be judged.

No meeting of the Association at Oxford could pass without reference to that famous occasion when its impact on public opinion was highest, when it was made quite clear that the pursuit of natural science would mean a painful revision of beliefs as well as a gain of material comfort, when it claimed acceptance for the theory that denied the special creation of mankind.

It was in 1860, the year after the publication of Darwin's book on ' The Origin of Species.' Bishop Wilberforce was to speak and Huxley was Darwin's champion. The audience was too large for the lecture room and they moved to the long west room of the Museum, but the first paper was dull, it was on ' The Intellectual Development of Europe considered with reference to the views of Mr. Darwin,' and after an hour of it the audience was restless and wanted something more dramatic. They had it from the Bishop, who spoke with eloquence and wit against the idea that man and the monkey could have a common ancestor. He ended with an ill-conceived joke about Huxley's claim that he was descended from the apes, but he sat down to general acclamation and the fluttering handkerchiefs waved by the ladies. Huxley had a more harshly coloured picture to present, the aim of life was merely to go on living, the road to progress was by the slaughter of the weak and the survival of the strong and man was cousin to senseless brutes. The scientists were demanding that the search must go on, that the evidence must be followed wherever it led, but until then it had not led to anything quite so bleak.

All this was no doubt implicit in Huxley's speech, but the audience at the British Association is human and what really stirred them was Huxley's grand indignation at the idea of a bishop condescending to such a paltry joke on so high a theme. His scorn forced the excitement to such a pitch that one lady fainted and had to be carried out, and afterwards no one, not even Huxley, could remember exactly what he had said. It did not matter so much, for the chief issue was no longer the origin of species, but whether the Bishop had really been guilty of a breach of good manners. But the dispute had been on the scientific plane and the evidence could not be ruled out because it was unpalatable : within a few years the battle for Darwin's theory was won and it seemed that the discoveries of science had forced the human race to give up beliefs on which it had relied for centuries.

Naturally the change came gradually, bringing discord into a good many families where the rising generation were zealous converts to the new creed. But now the scars are mostly healed. Advancing understanding of what was really at stake has made it possible for both sides to keep their ideals. The theory of evolution has lost its power to arouse passionate resentment or passionate faith.

I have mentioned that meeting for two reasons : because it emphasised that man himself was part of the natural world and because it emphasised also his right, at all events his determination to go on searching for knowledge, for without his curiosity he would be nothing.

We all know where our curiosity has now landed us ; with advances in atomic physics which might be applied to devastate half the world and if they were so applied would certainly make life in the other half extremely precarious. Our grandfathers here were faced with scientific discoveries which were no laughing matter, for to many of them they spelled the end of all worthy human aims, but we face discoveries which might spell the end of all human aims, worthy or not.

We can regret that atomic bombs are possible without regretting the discoveries that have led to them. Advances in natural science cannot avoid advancing the methods of warfare ; they do so when they make armies more healthy as well as when they increase the power of their weapons. But although the strategists have to think mainly of immense explosions and great devastation, it would be a

mistake to suppose that these are the only dangers. Even if we can survive then we must face the possibility that repeated atomic explosions will lead to a degree of general radio-activity which no one can tolerate or escape.

The level would not rise rapidly and there is a large margin in hand but the physicists can estimate the persistent contamination which must follow an atomic explosion of a given magnitude and the biologists can assign limits to the amount of contamination which could be let loose on the world without serious danger to every part of it. When atomic energy is used to supply power for industry the dangers of contamination are real enough, but due precautions can be taken to avoid them. In a major war they would soon be set aside. Powerful nations who think they could win quickly might accept the risk. A few hundred large bombs would not raise the level of radiation to the point where it would become a general danger, and no doubt a country of small area like ours could be reduced to ashes by a relatively small mass of explosion. Though the ashes would be deadly, the rest of the world might escape them. But a long war between powers well armed with bombs would certainly involve an order of radio-active contamination which would involve us all, victors as well as vanquished.

Arguments that war does not pay will not count much when ideals are threatened. It is true that a war which would probably end in wholesale destruction can appeal only to people who are desperate, but they can be made desperate and that is the end we have to guard against. There are conceivable safeguards, of course, but controls and conventions are not foolproof, and in future whenever the world is split into two opposing groups with large stores of atomic weapons it must face this added risk of catastrophe.

Yet we have surely no right to feel that our predicament is so much worse than that of earlier generations. Our grandfathers could do nothing to ward off the danger which seemed to face their cherished beliefs, but our fate is in our own hands. We are afraid, and rightly, because we cannot trust ourselves to act peaceably, because we know that unless we are ready to give up some of our old loyalties we may be forced into a fight which might end the human race. Our predicament is the inevitable result of our curiosity and of the physical nature of the world we live in, but if we can make our behaviour worthy of our increased knowledge we can live safely. The scientist, therefore, has a double responsibility. He must apply his science to learn as much as possible about the mental and physical causes which make us behave as we do, he must study human nature to prevent its failures ; but he cannot wait for the discoveries which might make us act more wisely : he must take us as we are and make it his task at meetings like this to point out that the human race cannot stand more than a few thousand large atomic explosions whether they hit their target or miss it. If we can make this known universally our Association will not have failed in its purpose.

It may be optimistic to think that our dangers would recede if we had a better understanding of human reactions : in fact if we must continue to make war there is no kind of scientific investigation which might not be used to make it more effective and there can be no guarantee that discoveries in the field of human conduct would be harmless. A drug or a system of education which would make us all do as we are told, a method of producing radical conversion to a new system of belief, a knowledge of new ways of rousing patriotic ardour, all these might be used with consequences almost as grim as the genetical deterioration in a radio-active world. The psychiatrist who discovers a cure for paranoia may find that he has also revealed a convenient way of producing it.

Our novelists have made us aware of these dangers, but it is some comfort to feel that in this case the increased knowledge could be used for defence as well as attack. We can only be protected from radio-activity by living in caves on uncontaminated food and drink, but an increased knowledge of how the mind can be influenced could certainly forestall many of the influences which might be used to undermine our integrity.

It is certainly true that discoveries relating to our own nature may mean a painful readjustment of our beliefs : that,

however, is a fair price for increased understanding and in fact our ideas about our own behaviour have already been assailed in such a way that further revelations are unlikely to shock us. There is a fairly close parallel between the impact of the theory of Natural Selection one hundred years ago and that of Freud's theories on our own generation. The British Association does not come into it, because Freud's evidence was all on the medical side, but his views made the same kind of attack on our pride and met with the same passionate resentment or approval. The theory of unconscious forces moulding our thought has certainly diminished our stature as intelligent beings ; yet the parallel still holds, for again we have recovered our equanimity. We are reconciled to the unconscious, though we may not have digested all the elaborations of psychoanalytic theory. We are no doubt less sure of ourselves, inclined to spare the rod and to put nothing in its place, but, on the whole, Freud has left us with a better understanding of human conduct and we are not down-hearted at finding it less rational than we used to suppose.

Freud would have liked to build up a system based on the physiology of the brain, but he was soon too deeply committed to the psychological side. Pavlov's conception of human behaviour was based on brain physiology and it was less disturbing because it did not go into such uncomfortable detail. It is now perhaps more disturbing than Freud's, because Pavlov's notion of the conditioned reflex has come to dominate one side of the world, but we must not think the less of it because it has been used to justify a political system foreign to ours. Pavlov and Freud were both scientists of surprising originality. They gave a new impetus to research on human activity, but the fields they explored are still waiting for the next advance to show how much they will yield.

The difficulty is that there are so many fields of inquiry to cover, each with its own limited range of facts and deductions. Freud studied dreams and neuroses and explained them as the product of repressed desires. Pavlov studied learning in animals and explained it in terms of conditioned reflexes, but physiologists ever since Galvani have studied the reactions of nerve fibres and nerve cells, the units of the nervous system, in the hope of explaining what they do in the terms of physics and chemistry. This approach at the lowest level can tell us little about the way in which units are organised, but when we keep to physical and chemical problems we are in the familiar territory of the exact sciences, we know how experiments should be conducted and there are great technical advances at our disposal. It is when we begin to think of organisms rather than molecules that we seem to part company with mechanism.

At this end of the scale then our actions are found to depend on the vast mass of cellular material which makes up the nervous system, receiving signals about the outside world from the sense organs and sending out signals to the muscles to produce the complex movements of intelligent behaviour. The nervous signals can be recorded and analysed because they are revealed by brief electrical effects and we are rapidly gaining a fairly clear picture of the energy transformations which make them possible (and incidentally we should never have gone so far if there had been no radio-active Sodium and Potassium for tracing ionic movements).

The sensory inflow brings information about the events taking place outside us and progress reports to show how successfully we are dealing with them ; signals from the muscles are needed to adjust the simplest movements and we are handicapped if we cannot hear what we are saying and cannot watch our step. But the great central mass of nerve cells has to fabricate a radically different pattern of messages to send out to the muscles and it is a pattern which depends on past as well as present information, on what happened to us a year ago as well as on what is happening now. Unfortunately, it is a great deal easier to study the immediate reactions of the nervous system than the more persistent changes which alter its habits and give us our memories. We know next to nothing about the plasticity which is the most important feature of the brain and that is the next hurdle for the bio-physicists and bio-chemists.

But we do know that the cells of the brain do not behave as passive agents for conducting and combining the signals that reach them. As long as we are awake many of them are in continuous rhythmic activity. The system has its own reserves of energy and is unstable, at all events it is so constituted that a slight disturbance of equilibrium will start up a cycle of discharge and recovery repeated many times a second and extended through much of its substance. It is no great surprise to find that we are driven to our daily activity by a cell system of this kind with energy to be dissipated, but we are now learning something about the interplay of the different parts of the system. It is found, for instance, that a relatively small collection of cells at the base of the brain has a profound effect on the general level of activity, so that we are aroused when it is stimulated and fall into coma if it is injured. We can see too how the chief focus of disturbance shifts from one region of the cerebrum to another when we transfer our attention. In fact we are beginning to trace a closer connection between what is going on in the different parts of the brain and what we happen to be doing from moment to moment. For what it is worth we can see a physical reason for our restless lives and our insatiable curiosity.

One may well feel that the most detailed knowledge of brain physiology will never help us much in our efforts to live peaceably, but it would be rash to prophesy. Certainly there are people now who lead more placid, if perhaps less useful lives, because their anxieties have been diminished by leucotomy, an operation on the frontal lobes of the brain. And long before the advent of leucotomy we had become accustomed to adjusting the activity of our nervous system by chemical agents. Tea and coffee, alcohol and tobacco are the stimulants and sedatives of the pre-scientific age and now, to quote the preacher, there is no end to the works of the apothecary, and we seem to be much less afraid of using the confections he gives us to take away our pains.

Only the writers of science fiction would suggest a future in which the problems of civilisation will be dealt with by tampering with the brains of some or all of mankind. It will help us a little to settle our differences if we have the means of ensuring a clear head and an even temper after a long journey and a resetting of the daily rhythm, but clearly we must look beyond physiology for an adequate picture of the human brain in action. Though it should start at the molecular or the cellular level, the evidence for it must include the activities of the finished product. We must find out what human behaviour is like before we try to explain how it is produced.

We do know, of course, an immense deal about human behaviour, from our own experience and from the accumulated wisdom of the past, but it is only in recent times that we have tried to check our knowledge by the methods of natural science. The development of physical science dates from the time when direct observation and experiment were accepted as better guides than the principles which had seemed self-evident to the philosophers and the schoolmen. They were wise enough but it was found that they could be mistaken. And so now we can look to the many branches of social science to make a dispassionate study of what actually happens in our society without regard to what might be expected to happen if we are to believe all we have been taught.

The picture of human behaviour which the social scientist has to draw is of a system in which the units are men and women rather than cells or molecules. It is true that one man behaves very differently from another—it is part of our political creed that they must be allowed, within limits, to differ as much as they like. But, although we can insist that the units are not all alike, the general principles which determine their behaviour ought to stand out when we deal with millions rather than individuals.

There is, in fact, one branch of social science which can adopt this plan without difficulty. This is the science of economics which considers only the human activities of producing and consuming and studies the way in which these activities are to be balanced. When the balance is lost credit and currencies fail and we may blame the economists for the plight we are in, but the status of their science is unquestioned,

and no one would dream of saying that our complex civilisation could have done as well without them.

It must be admitted, however, that the strength of economic theories rests very largely on the fact that they can be worked out with very little regard to human nature. Men must be assumed to be capable of trading with one another and they must have a variety of skills and needs and possessions, but that is all, or nearly all, that the economist has to consider. Few of the many branches of social science can proceed on such a simple basis and it is because of this that the subject as a whole has still to win full recognition in this country where the science of economics is so firmly established.

Theories describing or explaining other kinds of social activity are nothing new, they existed long before the theories of economics ; but the economists have had the figures to check their conclusions and until recently the sociologists have had little but their own philosophy and their own reading of history. Even Durkheim, who broke away from the philosophic tradition, could only point the way to a truly objective study of human society. Now the position is quite different. A century and a half ago it had only just been decided that the population of this country was on the increase. Many had thought that might be so but there were no figures to show whether they were right or wrong. Now there are all the modern techniques for fact finding, the questionnaires, the punched cards, the sorting machines and the statistical methods. It is far easier now to deal with large groups and the psychologists have far more knowledge of the irrational factors which can sway the smaller groups as well as the family and the individual. The stage seems to be set for the new development and it might well be the most important scientific development of this century. Why are we still so reluctant to think well of it ?

The answer, I suppose, is that we are not yet convinced that the kind of observations that the social scientist can make will be sufficiently objective and sufficiently precise. Those of us who work in laboratories have a far easier task in selecting what we should observe, yet we know how difficult it is for us to select and observe fairly. We have to school ourselves not to reject the exceptional result as worthless when it does not fit a cherished theory : we have to be continually aware of our own fallibility even though we have all the figures and controls to keep us straight. We are loth to believe that the social scientists are more open-minded than we are and the material they have to deal with seems to need an almost superhuman open-mindedness combined with an almost superhuman power of selection, of seeing the wood as well as the trees in it. We feel that we should be lost in such a wood where everyone must feel the bias of his own up-bringing and social ties, where there is so much that cannot be measured and may or may not be relevant and where there is rarely any opportunity to check the conclusions by experiment.

Our distrust is probably intensified by the layman's tendency to speak of experiments in the social field as though they were comparable with the experiments which obey all the exacting rules of the laboratory. There we can at least hope to proceed by changing one factor at a time. The social scientist would be the last to cherish any such hope : his whole training warns him of the complexity of any situation where human beings are involved. But many people seem to think that if something in the social field is done in a new and usually more expensive way we have only to call it an experiment to justify the conclusions we wish to draw. Certainly we must try new methods and hope to find out why they succeed or fail, but although a new way to check juvenile delinquency or develop a housing estate may give the most favourable results, it is very seldom an experiment from which one can infer the precise factors that have made for its success. A change in the birth-rate, a wet summer or a newspaper campaign are the kind of disturbing element which would be too obvious to mention were it not that everyone who has worked in a laboratory must be aware of having overlooked disturbing factors which should have been just as obvious. We were lucky if our control experiments saved us from exposing our folly, but controls are far easier in the laboratory than in the world outside.

This is a minor grudge. If we harbour it we shall be visiting the sins of the enthusiast on the very people who exist to keep them in check and we must surely welcome any new branch of study which tries to substitute the methods of natural science for unchecked speculation. It is very easy for us to forget our own rules when we allow our feelings to take charge, to say that a result was too obvious to be worth the proof if we happen to like it and if we dislike it to say that statistics can be made to prove anything. Perhaps we have forgotten how much we distrusted some new development near to our own field of science because it was unfamiliar and because we thought its backers claimed too much for it. We ought to remember that the now flourishing science of bio-chemistry was once distrusted by chemists as well as physiologists. It is human nature for the guild of natural scientists to delay admitting a new member till he has paid his dues and satisfied the examiners of his competence in the craft.

At present there are many kinds of investigation grouped under the umbrella of social science : the groups seem to have little in common and few of them can put their results into figures, but I think all of us, scientists and laymen alike, are becoming more aware of the value of social investigation and of the degree of certainty it can bring. We may have distrusted the army psychologists who classed us by our aptitudes, we may have read the Kinsey report from unscientific motives and we may continue to allow the accident prone to drive their cars. We can see none the less that there are facts to be found out about our usefulness in society, about our relations to one another and about the risks we take. We can see too that the search for these facts can be conducted on reasonably scientific lines. It is too early to be cautious in spending money on large scale investigations. They are bound to be costly and those of the social scientist deserve not only the support of national and international funds. There is this kind of support for the subject already, but it is too important a plant to be left in the hot-house atmosphere of research institutes and Unesco teams. It deserves to be in full contact with all the conservative and academic people in universities, the lawyers and historians as well as the economists, biologists and statisticians. There must be more social scientists in our universities so that the rising generation can see what they are like.

I have put in this plea for a subject about which I know so little because it seems to be developing more rapidly in other countries, particularly in the United States. We are handicapped, no doubt, by smaller resources and perhaps by the remains of a national temperament which has made us prefer to work by ourselves and not as members of a large team. We ought not to expect too much. Social scientists like economists may be able to foretell the consequences which are likely to follow in a particular situation, but the statesman who goes to consult them may come away with little to comfort him and may turn to the quack doctor in the next street. We may find out a great deal about the tensions which lead to war without seeing the way to keep ourselves clear of it.

But human beings, when we consider them as material for the biologist, are not to be thought of as incapable of improvement. Other kinds of animal have been found to possess unexpected power of communicating with one another, but we are the one kind endowed with a brain which gives us the power of communicating by putting our impressions into words and appreciating the meaning of the words we hear. Speech would be little use to us unless we could remember what it meant, but memory, the ability to learn, is a property of the simplest kinds of nervous system. We alone possess a nervous system which gives us the power to order our ideas in words. We alone have this way of thought which allows us to compare a new problem with an old one.

With this unique equipment for thinking and communicating we can form our habits not only from what happens to us personally but from what happens to our fellows and from what has happened to countless generations in the past. And although the storage capacity of a single human brain is limited we have learnt to make permanent records of what has occurred, in visible symbols, so that what cannot be remembered can be found in a book. In this scientific era our store of

knowledge is growing so fast that we shall soon need new ways to keep it available. Books have done duty for a thousand years and we should be sorry to lose them, but we can change our habits rapidly and it is already old-fashioned to write a letter with a pen.

This increasing background of experience has meant that we are constantly acquiring new habits and new ways of thought. It does not take us very long to see the way round old quarrels. Darwin and Freud no longer trouble us. We are no doubt born with brains like those of our remote ancestors and when we are grown up we have no more native intelligence than they had, but our brains must have been so modified by what we have learnt that they are physically and chemically different, better adapted for the complex social life of our time. We have more knowledge at our disposal. If all goes well with our training the brains we have ought to be more civilised than those of our fathers and those of the next generation more civilised than ours.

I have claimed that the scientific investigation of mankind can help the process of civilisation by finding the weak points in our equipment and suggesting remedies, but these scientific activities will play only a limited part in the development of human society in the scientific age. The power we have acquired over the forces of nature has made it possible to increase our mental training as well as our standard of comfort. Of the two or three thousand million people in the world perhaps not more than five millions are receiving a full university training though no doubt more are trained in a narrow technology. Yet the number has risen steeply in spite of wars, perhaps even because of them, and it continues to rise. In the United Kingdom we have 85,000 students at our universities, about 1 in 30 of the whole age group and that may be all we should contemplate with our present system. A few years ago, however, it was only 1 in 60, and there must be many parts of the world where the university, as we know it, is only now beginning to play its part in civilising the most intelligent citizens. This could never have happened without all the scientific inventions which have been blamed for our troubles, the improved transport, the cheap printing, the electricity and the internal combustion engine. And a university training would have been far less civilising if it had never left the old authoritarian pattern which roused Huxley to speak in Oxford nearly one hundred years ago.

University students, however intelligent, are not usually considered to be the most peaceful members of the community. They have been more welcome in small country towns than in the capitals where they can join revolts against the government of the day, and they tend wherever they are to be critical of those in authority. Long may students remain so. If they were not, if they believed all they read in an officially inspired press, or even what they were taught by unrestrained professors of the greatest integrity, there would be little chance of their learning how to use their knowledge for solving the new problems of our time. The plodding methods of the laboratory and the card index must be there to check their enthusiasm and to show them how the problems have come about. Even if they get no help from that it is something to know that there are many more people in the world to-day with brains trained better to deal with their environment by learning how its problems have been dealt with in the past.

Our Association is concerned with the advance of natural science. It began when we had little control over the forces of nature and we have now so much that we might soon become able to destroy two-thirds of the world by pressing a button. Yet the control which has been achieved by science has made it possible for us to improve our own natures by more education in the arts of civilised life. We may perhaps improve ourselves more rapidly if we can gain more insight into human behaviour. That is something which the Association can encourage, but it is only a small part of what it must do. It must not cease to encourage every kind of scientific inquiry, for it is human nature to inquire, to learn by experience, and to profit by what it finds out.

RECENT DEVELOPMENTS IN NUCLEAR PHYSICS

Address by

SIR JOHN COCKCROFT, K.C.B., C.B.E., F.R.S.

PRESIDENT OF SECTION A

I AM going to give some account today of the remarkable developments in nuclear physics which have occurred since the war, developments as exciting as those in an earlier epoch when Rutherford was the President of Section A at the Winnipeg meeting of 1909.

This was the time when Rutherford's nuclear atom was being conceived. He spoke of the experiments of Geiger and Marsden in which high-speed alpha particles were scattered backwards from thin foils, leading to the conclusion ' that the atom was the seat of an intense electric field ' which was responsible for the reversal of direction of these particles.

Fifteen months after this he produced his nuclear model of the atom. The model accounted in detail for the scattering of the high-speed alpha particles in the inverse square law electric field of the nucleus. Rutherford's scattering law gave the relative number of particles scattered through different angles. This was found to give a good account of the results and enabled an upper limit to the radius of a heavy nucleus such as gold to be fixed as $3{\cdot}2\times10^{-12}$ cm.

The next address of Rutherford to the British Association was his Presidential Address at Liverpool in 1923. He then referred to his experiments on transmutations of light elements by alpha particles. He considered atomic nuclei to be composed of protons and electrons—' occupied but not filled by these minute building units in ceaseless rapid motion controlled by their mutual forces.' He was not at this time as certain as he had been 20 years earlier ' that the atoms of all the elements contained a great concentration of energy which would be available for use if only some simple method could be discovered of promoting and controlling their disintegration.' He referred, however, to the recent measurements which had shown the mass of the helium nucleus to be appreciably less than that of four hydrogen nuclei and the consequence ' that the transformation of one pound of hydrogen to helium could release as much energy as the complete combustion of 8,000 tons of coal.' This had led Eddington and Perrin to their conclusion that the synthesis of helium was the main source of stellar energy. We have recently had several manifestations of this process of synthesis.

These two Presidential addresses are the last which have been devoted to nuclear physics at the British Association. During the remaining years of Rutherford's life our concept of the internal construction of the nucleus was drastically changed by Chadwick's discovery of the neutron. This led to a more satisfactory nuclear model in which neutrons and protons are the elementary bricks of the atomic nucleus. The nuclear transmutations by alpha particles which Rutherford had first discovered were extended in an enormous way by the use of protons, deuterons and neutrons as transmuting particles, and by their aid radioactive forms of most of the elements were discovered. I had the good fortune to take part in this work. Rutherford did not live to see his 1905 vision of large-scale release of atomic energy realised, for nuclear fission was not discovered until a year after his death. We finished the war, however, with a staggering demonstration of the magnitude and the power of nuclear forces, together with some promise of peaceful applications.

But we had still little knowledge of the true nature of these forces. It was clear enough, however, that the assembly of protons and neutrons constituting a nucleus could only be held together against the enormous repulsive forces of the protons if there existed powerful attractive forces between nucleons of a short-range character. It was also clear that this true nuclear force of attraction must occur almost equally between pairs of protons, pairs of neutrons, and neutrons and protons.

Nuclear physicists returned from their war service not only to follow up the great practical applications of nuclear energy but to delve still deeper into the nature of these mysterious forces. They returned with new electronic techniques born of radar experience and with a competence in machine building which enabled particle accelerating machines to be built for producing protons, electrons and X-rays of energies in the region of 100 to 400 million volts. Within the last year we have brought into use in our universities a 330-million-volt electron synchrotron in the University of Glasgow, a 400-million-volt proton accelerator in the University of Liverpool, and a billion-volt proton synchrotron in the University of Birmingham. Recently accelerators of 2 and 6 billion volts have started to work in the United States, whilst a further step to 25 billion volts is now envisaged by the European Nuclear Research Organisation at Geneva. These new tools provide the means for exploring the reactions of high-speed nucleons with nuclear matter. To these machines were added the complementary techniques of the cosmic ray physicists, interested first in exploring the nature of the cosmic ray particles and phenomena, but finally, through their photographic emulsion techniques and cloud chamber photographs, leading the van of discovery in high-energy nuclear physics.

The first interesting group of experiments are the post-war analogues of the Geiger-Marsden alpha particle scattering experiments referred to in Rutherford's Winnipeg British Association address. The 8-million-volt alpha particles from radium are replaced by 140–180-million-volt protons from the Harwell cyclotron or 200–450-million-volt protons from one of the giant cyclotrons in the United States.

Experiments were first carried out on the scattering of nucleons by individual nucleons—protons were scattered from protons and then from neutrons—in the hope of learning something of the nature of these nuclear forces.

A beam of 145 MeV protons was extracted from the Harwell cyclotron and passed down a 25-foot length of pipe through a thick shielding wall into the experimental chamber. The protons strike a target of polyethylene containing hydrogen and carbon atoms and the scattering by the hydrogen nuclei is studied, appropriate corrections being made for scattering of the carbon nuclei. The scattered protons and the struck protons are counted by modern versions of the simple zinc sulphide scintillation screen used by Geiger and Marsden. This is now replaced by new types of scintillating crystals or liquids which are observed by a photo-electrical multiplier having a capacity for counting very fast compared with the sixty a minute which was the most the visual observer could do in the old days. Three of these crystals are used in a line to form a ' telescope ' which records when a particle passes through all three. The energy of the particles which are recorded is measured by inserting an absorbing cylinder of graphite between the last two crystals, so that only a particle energetic enough to traverse this thick cylinder produces a record in the ' telescope.'

Measurements have also been made of the scattering of 102 MeV neutrons by protons. A liquid scintillation counter was used to detect the neutrons. The diffusion cloud chamber has also been used for this experiment.

These results, together with those on proton-proton scattering, show that the angular variation of the scattering is very different in the two cases. The scattering of protons by protons is independent of angle. The neutron-proton scattering has high values at small angles and at 180°, in centre of mass co-ordinates which corresponds to very small angle scattering and scattering through an angle of 90° in laboratory co-ordinates.

We would expect a large number of neutrons to be scattered through small angles since the transfer of momentum at

these high energies is likely to be small. For this type of glancing collision the struck proton would come off at right angles. But here we find a maximum in neutron numbers. This means that in these collisions neutrons and protons change identities. This is known as exchange scattering—a second characteristic of nuclear forces.

Apart from this, the general character of the scattering curves could be explained by the fact that we are dealing with two identical particles in the one case and two different particles in the other; the theoretical physicists also had to assume either that the nucleons had a hard core giving rise to strong repulsive forces at short range and attractive forces at longer range; or, alternatively, that there is a strong interaction of the spin of the nucleons and their orbital angular momentum, due to magnetic coupling. But these models are only partly successful in explaining the experimental data and the situation is still rather unclear.

Some new experiments carried out at Rochester, Harwell, Chicago and Berkeley have provided more evidence for the nature of the forces. In the Harwell experiments the beam of 150 MeV protons is allowed to strike a carbon target inside the cyclotron box and those scattered through an angle of 20° are selected by a slit and are brought outside the vacuum chamber, passed down a long pipe and then scattered a second time from a carbon target. The numbers scattered through a given angle left and right and up and down are counted. When attention is focused on the protons which are elastically scattered on the second collision, the numbers scattered to the left are found to be four times more than the number scattered to the right.

When the experiments are carried out with hydrogen as a second scatterer instead of carbon, the number of protons scattered to the left is 1·5 times the number scattered to the right.

The numbers scattered up and down are equal. These results can be explained by the hypothesis that after the first encounter with carbon nuclei about 80 per cent of them come out of the collision with their spin axes pointing in a direction normal to the plane of scattering, the amount of polarisation increases strongly above 100 million volts.

Similar though much less marked effects have been found in the double scattering of neutrons.

It is rather early yet to be sure of the theoretical explanation of these polarisation effects. However, Schwinger eight years ago suggested that such effects might be produced by interaction of nucleon spins with the orbital angular momentum. Thus, when the spin of the nucleon is in the same direction as the vector representing the orbital angular momentum, there would be a lowering of the energy of these nucleon orbits due to magnetic interaction and they will be favoured. Fermi has recently calculated the magnitude of the effect and been able to give a good account of the experimental results. He predicts that protons coming out of the first collision with their spins up should be scattered more to the left than to the right.

For those to whom this picture means very little it may be comforting to produce a classical analogue.

If we first consider the collision of two spherical bodies having rough surfaces then the collision would result in some rolling and imparting of spin to the particles. Translated to a quantum picture this corresponds to an orientation of existing spins.

The second encounter of the now spinning particle with the nucleus would then result in more going to the left than to the right, due to the spin—just as spin imparted to a billiard ball biases its deflection.

Although the classical picture is only interesting as a visual model, it is nevertheless interesting to notice that the friction force in this classical picture is a non-central force. The spin-orbit force the theoretical physicist wishes to introduce has a similar character.

Another important and older group of experiments throwing light on nuclear forces showed that as the proton energy is increased beyond 200 MeV, a short-lived type of particle, the π meson, is created in the intense field of the collision. The simplest case occurs when the high-speed protons enter a liquid-hydrogen target. Protons then combine with protons to form deuterium and a positive π meson is

created. These π mesons were first discovered by Professor Powell as a component of the cosmic rays, where they are created by nucleon collisions. They live for $2 \cdot 10^{-8}$ of a second and decay into μ mesons of lower mass which in turn decay into electrons and neutrinos after $2 \cdot 10^{-6}$ second.

In collisions of high-speed protons with other nuclei such as carbon, both positive, negative and neutral π mesons are created. The negative mesons interact strongly with nuclei and produce nuclear explosions; the neutral π mesons decay after a life of 10^{-15} of a second into two γ-ray quanta.

This strong interaction of the π meson with nucleons and its creation in nuclear encounters realises a prediction made in 1937 by Yukawa. The π meson seems to play the same role in nuclear forces as that played by the photon in electrodynamics. In electrodynamics the electron is the singular source of the electromagnetic field which is described by Laplace's equation whose well known solution leads to the static field $1/r^2$ for the field of force of the electron. The nuclear forces must be described by a wave equation rather similar though different from Laplace's equation—different mainly because of the need to introduce a term expressing the very short-range character of the nuclear forces. This new term introduced by Yukawa led him to predict the existence of a meson which was to be responsible for transmitting the nuclear force between neutron and proton. Thus, when a proton approaches a neutron—the positive meson cloud surrounding the proton transfers itself to the neutron, which then becomes a proton. In this process momentum is transferred and the particle changes direction. In a proton-proton or neutron-neutron collision the interaction is conveyed by the neutral meson as was first postulated by Kemmer. The mesons are thus coupled to the nucleon and are called into play in collisions to transmit the forces. They actually materialise in very violent close collisions.

A recent refinement of the meson theory of nuclear forces by a young French physicist, Levy, using what is known as symmetrical pseudo-scalar meson theory, reproduces the low-energy nucleon-nucleon collision behaviour quite satisfactorily and it suggests a strong repulsion at close distances. This gives one of the possible explanations why nuclei do not collapse.

The coming into operation of the new accelerator of Brookhaven has provided protons of energies up to 1·3 billion volts. In this energy region mesons of still higher mass materialise, live for a fraction of a milli-microsecond and decay into lighter mesons of already well known types.

Perhaps the most striking event followed the creation of a 1·2 BeV positively charged π meson by 1·3 BeV protons striking hydrogen. The energetic π meson was observed, in a cloud chamber of the continuously sensitive type filled with hydrogen under pressure, to stop, presumably striking a hydrogen nucleus. A little farther along, two pairs of V tracks appeared. One V consisted of a proton and a π meson with kinetic energies of about 37 million volts. These are the well-known decay products of the Λ° meson discovered by Rochester and Butler in 1948. This heavy meson, which seems to be an excited nucleon, is one of a family of mesons heavier than nucleons which have recently been named hyperons. It has a mass of 2182 times the electronic mass and a lifetime of $3 \cdot 7 \times 10^{-10}$ second.

The other V track consisted of two π mesons, one positive and the other negative, having a kinetic energy of 214 MeV. These are the decay products of another heavy uncharged meson, now known as the θ° meson, of 966 times the electron mass and a lifetime of $1 \cdot 5 \times 10^{-10}$ second.

The primary event, the striking of a proton by an energetic π meson, thus created two heavy uncharged mesons, the Λ° and the θ°.

Besides the four mesons I have so far mentioned, there is the well-authenticated τ meson which is created in nuclear collision and decays into three π mesons having a kinetic energy of 74 MeV. This τ meson has a well-established mass of 975 times the electron mass. There is also a $\varkappa$ meson with 1,210 times the electronic mass which decays into a μ meson directly, and at least five others whose authenticity is not so certain. These heavier mesons must also contribute to nuclear forces.

Very recently the cosmic ray workers, who are almost always first to discover new mesons and new mesonic phenomena, have found that the excited nucleons or

hyperons can be created *inside* nuclei in energetic collisions. Dansyl and Pniewski observed in a photographic plate exposed to cosmic radiation a heavy nuclear fragment of charge $5e$ emitted from a nuclear disintegration. At the end of its range, after a life of about $3 \cdot 10^{-12}$ second, when the velocity was practically zero, it disintegrated with the emission of three or four charged particles. It might have been thought that this was a spontaneous disintegration of a nuclear fragment which had been highly excited by a nuclear collision. But in this case it should have disintegrated in less than a millionth of the observed lifetime. Dansyl and Pniewski suggested therefore that a hyperon or excited nucleon might be present in the fragment and that its disintegration gave rise to the observed energy release.

Since that time several more events of this kind have been observed in photographic plates and in these cases the energy release was about 40 MeV, consistent with the view that a hyperon had disintegrated into a proton and π meson. In particular, a fragment of about the mass of the triton was observed at the end of its range to disintegrate into a π meson and what was probably a He^3 nucleus with the kinetic energy release characteristic of a hyperon decay. Nuclei may therefore contain for short periods after their formation excited nucleons together with ordinary nucleons.

We have seen then that atomic nuclei are bound together by the nucleon-meson field, just as atoms and molecules are bound together by electric and magnetic fields. The mesons may be thought of as transient fluctuations and concentrations in the nuclear field and we may have to think of separate fields for the different mesons. The heavier mesons will give rise to forces of shorter range than the π meson and will only be important in the more energetic collisions.

Very recently Professor Heisenberg has developed an interesting theory in which elementary particles appear as discrete stationary states of non linear field equations which describe matter in a very general way.

Experiments with Mesons

More information on the nature of mesons is being obtained by experiments with the meson beams which can now be produced by large cyclotrons. The Chicago cyclotron can direct about 1,000 mesons a second through a one-inch pipe passing through the shielding wall. I will mention two typical experiments on mesons.

The first of these was designed to determine the spin or intrinsic angular momentum of the meson. It is important to know whether it has zero spin like a light quantum or alpha particle, or a spin 1/2 like an electron or a nucleon, or a still higher spin. This can be done by taking account of the fact that in their creation processes, particles with the higher spins could be created in several states corresponding to different spin orientations and this increases the chance of their creation.

So an experiment was devised in Chicago to measure the relative probability of the creation of a π^+ meson when protons collide with protons and the reverse process of the absorption of a π^+ meson in deuterium. The creation process will occur three times as strongly if the meson has spin 1 than if the particle has spin zero. A further experiment shows unambiguously that the π meson has zero spin, which makes the theoretical physicist happy.

The second meson experiment studied their scattering by nucleons in a similar way to the experiments on scattering of nucleons by nucleons.

These experiments carried out in Chicago showed that the π^+ mesons were scattered from liquid hydrogen more strongly in the backward than in the forward direction. The π^- mesons, on the other hand, were scattered elastically more strongly in the forward direction. Some of the π^- mesons, however, changed on scattering into a neutral meson and in this case they were scattered more strongly backward. This peculiar behaviour of the mesons has been explained by assuming that on approaching the proton a compound nucleus of very high excitation is found and that scattering behaviour is due to a resonance phenomenon.

Other experiments study what are known as mesic atoms. Both π^- and μ^- mesons have been shown to form Bohr orbits round nuclei—the orbits will be of smaller radius than electronic orbits because of the larger meson mass compared

with the electron. Having formed the mesic atom, transitions between the Bohr orbits occur, giving rise to X-rays. The energies of these X-rays have been studied and agree remarkably well with theoretical predictions provided the radius of the nucleus is taken to be slightly smaller than the value obtained in proton scattering experiments.

Experiments with mesons will be greatly helped by a still more copious supply of these particles. We are therefore building at Harwell for use by British physicists generally, in a separate open area of the Establishment, a linear accelerator for producing 600 MeV protons. These protons will bombard a liquid-hydrogen target and will, we hope, provide us with 1,000 times as many mesons as are at present available. A long vista of interesting experiments has been drawn up in collaboration with university physicists who are helping with its design and construction.

I can only refer very briefly to the important post-war developments in what might be described as classical nuclear physics—the study of the structure of atomic nuclei by the use of nuclei of energies up to about 20 million volts.

From this we now have clear evidence for the building up of ' shells ' of protons and neutrons in nuclei leading to singularities in nuclear properties as the shells are filled. Thus the abundance of nuclear species with closed shells is higher than normal ; they have more stable isotopes ; their structure of excited levels is simpler. All this can be explained by invoking the type of nuclear force I have discussed earlier—the force which is characteristic of strong interaction between nuclear spin and orbital momentum. The energy levels can be calculated and filled up progressively rather like atomic shells are filled up, and the shell picture emerges.

Nuclear structure differs, however, from atomic structure because there is no counterpart to the overriding strength of the nucleus in determining the electron orbits. So the collective motions of nuclei become important and they can have vibrational and rotational motions which can be checked experimentally.

I ought not to close without saying something of the dangers and the potential benefits which have arisen from the exploitation of these powerful nuclear forces. The President has already spoken of the dangers which might arise from full-scale atomic warfare.

The destruction and danger from heat and blast and radioactive fall-out in the immediate neighbourhood of the large explosions is by now apparent to all.

Our Prime Minister has referred to the danger of world-wide radioactive contamination and has said that the biological aspects of these experiments require profound study.

In this study we have to take account of the conclusions of the geneticists on the effects of radiation in increasing the natural mutation rate. Dr. H. J. Muller's 1952 address to the Stockholm Conference on Radiobiology and Radiation Protection has summarised the present views of geneticists and has suggested how much radiation the human race can tolerate without harm. The present contamination is slight and very far from this limit. Our measurements will in time tell us where the danger-level lies.

What is certain is that a full-scale atomic war would be a disaster for the whole world. Our Prime Minister has spoken of our present situation as one of great danger and measureless opportunities. We must hope and believe that all Governments will take the same view when they see the danger and unite to prevent it happening.

If mankind can achieve this, then the great power of nuclear forces can be used to his great future benefit.

I have taken part almost from the beginning in the great adventure of atomic energy development. We had only just begun to think about the consequences of Hahn and Strassman's discovery of fission when war came upon us and for a time engulfed us in more immediate and practical matters such as radar. So, for a time, I was an interested onlooker, gradually becoming more and more convinced of its importance, until I was translated to Canada and joined the group who were starting to design the Canadian heavy-water piles. The larger of these piles became for a time the most powerful source of neutrons in the world. This has enabled the Canadian project to play a

leading part, under the guidance of Dr. W. B. Lewis, in the development of nuclear technology. In this country, after building from the beginning in 1946 facilities for work on atomic energy, we have embarked during the last three years on a vigorous programme of nuclear power development.

The achievement of economical nuclear power depends on three main factors. The nuclear reactor must be able to burn up a sufficiently high proportion of its uranium fuel to achieve low fuel costs. This depends not only on favourable values of nuclear constants, but also on a reactor design leaving as many neutrons as possible for production of new fuel from otherwise inert materials such as U238 and thorium; engineers who waste neutrons can easily offset the bounty of Nature. Nuclear technology is the second important factor. We must be able to prepare uranium fuel cheaply, and its metallurgical properties must be favourable to really long life in a reactor; the chemists and chemical engineers must process our spent fuels cheaply and return any valuable secondary fuels such as plutonium for reburning. The third factor is good engineering design, leading to reliable and safe operation and low capital costs.

In all these fields we have seen steady progress, more favourable than we dared to hope for in the early post-war years. As a result of this, in Cumberland the first large-scale power station using nuclear energy as a source of heat is being built by our colleagues of the Atomic Energy Industrial Group, following a Harwell design study. This first nuclear power station has been described by Sir Christopher Hinton in his Thomas Hawksley lecture to the Institution of Mechanical Engineers. Its design is an extrapolation of our post-war experience with graphite piles at Harwell and Windscale. It should provide us with invaluable first experience in practical nuclear power. We can already see our way to great improvements in nuclear power stations of this kind and they are likely to make an effective contribution in filling any gap which may arise in the next twenty years between our energy needs and our supplies and should be able to produce power a little above conventional costs. But we can also think of an embarrassingly large number of types of nuclear power units which should use nuclear fuel much more efficiently and which would repay investigation, and of course they look more and more attractive the farther off they are.

Energy is the essential basis for a highly developed civilisation. The application of nuclear fission, wisely guided, can ensure that for a millenium ahead mankind has all the energy needed to supply his ever-growing needs. Before we come to the end of our uranium fuel reserves, I feel sure that the energy of fusion of the light elements will also be turned from destructive to peaceful uses and so provide a source of power without limit.

NEW IDEAS IN CHEMISTRY

Address by

SIR JOHN LENNARD-JONES, K.B.E., F.R.S.

PRESIDENT OF SECTION B

THE INTERPLAY OF THEORY AND EXPERIMENT

IT is the purpose of this Address to describe some of the new ideas that have been introduced into the theory of molecular structure within recent years. These ideas have been drawn from physics, where they have been singularly successful in correlating the properties of atoms. The scope of the theory has been widened so that the physical and chemical properties of both atoms and molecules can be explained by the same unifying hypotheses. As the range of ' explanation ' of the facts of nature extends, the more mysterious become the properties to be ascribed to the ultimate particles of which matter is composed.

In the progress of science there are two processes at work. In the one there is the steady accumulation of factual knowledge, and in the other there is an attempt to interpret the facts in terms of unifying principles. It is the interplay of theory and experiment which leads to the most important advances, for often the interpretation of one sort of fact leads to the prediction of new results. The most impressive advances are produced when general hypotheses are put forward which bring different branches of science within the same comprehensive scheme.

There has been no more striking example of this than the recent unification of atomic physics and chemistry. When the British Association last met in Oxford in 1926, there seemed little relation between the subjects discussed in the section of physics with those discussed in the section of chemistry. The Presidential Address in the one dealt with the properties of atoms in absorbing and emitting light, that in the other with the scope of organic chemistry. It did not seem possible at the time that both domains of knowledge might soon come within the same theoretical framework. Yet even during the Oxford meeting there were rumours of a new theory of the atom which was said to be brilliantly original. We did not know then that this new theory was to let in a flood of light on the behaviour of molecules and illumine much that was obscure in the world of chemistry.

THE ORBIT THEORY OF ATOMS

In 1926 electrons in atoms were pictured as rotating in orbits round a central nucleus like a solar system. To fit the known properties of atoms it had been found necessary as well to assume that electrons had a magnetic moment. They were said to have ' spin ' and the axis of spin could set itself along or opposed to any applied field. The spin had thus two possibilities only.

On this basis Bohr had made an advance of great importance to the chemist by suggesting how the electrons in atoms should be assigned to the available orbits so that their chemical behaviour might be explained. The striking achievement of this theory was to provide an understanding of the similarities in the behaviour of atoms assigned by chemists to the same groups in the Periodic Table. The Bohr theory was greatly simplified by Pauli when in 1925 he proposed an important new property of electrons in matter. According to this only two electrons in an atom can follow the same orbit and of these one must have one kind of spin and the other the opposite spin. In other words, no two electrons in an atom can simultaneously have the same spin and occupy the same orbit.

A Mysterious Property of Electrons

The full significance of Pauli's hypothesis, now universally called the Exclusion Principle, was not fully realised when it was first announced. Its main purpose was to simplify the Bohr description of atoms, or to suggest simple rules by which the same results could be achieved. But it has since become clear that it is of wide validity. It applies not only to atoms but also to the totality of electrons in a molecule, however many atoms that molecule may contain. Though first put forward as a guiding hypothesis in physics, it has become the corner-stone of chemistry. However complex a molecule may be, however large the number of electrons it may contain, there seems to be an ' awareness ' by each electron of what all the others are doing and a strict adherence to the rule that no two must play the same role at the same time.

No theoretical proof has been given of this exclusion principle. It is injected into current theories as a working rule. It represents some deep property of electrons, and indeed of all matter, which is not understood. It implies an intimate interconnection between the constituent parts of molecules and between one molecule and another. The principle suggests that there is a connecting strand running through the material universe, so that strictly no part can be isolated—except in an idealised sense—from the rest. There are always influences at work conditioning the behaviour and properties of the part and connecting it with its environment. These influences are more subtle than forces of repulsion or attraction such as exist between electrical charges of like or unlike kind. These latter are of short range and cease to be effective beyond lengths of atomic dimensions, which are infinitesimally small. But the exclusion principle applies to all the electrons of a long threadlike molecule or a long strip of metal, so that each is influencing the behaviour of the rest over distances which are millions of times greater than the sizes of the atoms which the metal contains. There seems here to be some aspect of nature of great philosophic content beyond the understanding of the physicist. He must be content to accept the implications without hoping to penetrate the mystery which is implied.

New Ideas

The Bohr atomic theory made clear the significance of complete shells of electrons in atoms and explained the tendency of some atoms to lose one or two of their outer electrons and of others as easily to assimilate them. Thus the type of bond known as the electrovalent link became understood in terms of the inner structure of atoms. Physics had successfully invaded the realm of chemistry. But the penetration was incomplete. The theory gave no indication why atoms of the same kind should combine and so the whole field of organic chemistry was left untouched. It was the new theory of the atom which opened the way.

Stimulated by de Broglie's suggestion in 1924 that electrons, indeed that all matter, have wave properties, Schrödinger developed what has now become known as *wave mechanics*. This is to be regarded as a generalisation of Newtonian mechanics, for it leads to Newton's laws when applied to heavy particles. There cannot be said to be a proof of the wave equation, for it was obtained by analogy and its justification rests only on its success. The wave equation must be accepted as an article of belief or a fundamental postulate. It involves some strange ideas and some processes of calculation which cannot in detail be understood. Thus the solution of the wave equation for any problem is expressed as a function of the coordinates of the system and of the time, usually denoted by ψ, but this function is merely a means to an end. It is not possible to give to it any simple interpretation in terms of observables. It is to be looked upon rather like a ghost which produces material manifestations. One simple connecting link between ψ and observation is that for a given system the square of the amplitude of ψ, for a particular set of coordinates, gives the probability that a system has a particular configuration, specified by its coordinates. Rules have been devised whereby the probable values of dynamical quantities can be derived from ψ and so the theory can be tested by comparison with experiment. In particular, the theory leads to a set of stationary states with definite energy values for atomic and molecular systems.

The wave theory of atoms was the first

new important idea since the Bohr theory to prove of importance to chemistry. It was found possible to use with it the idea of electron spin and the exclusion principle as well. But the new theory brought other ideas in its train.

So far as we know, electrons are indistinguishable from one another. An interchange of any two in a molecule does not produce an observable change. This means that the probability distribution function of ψ^2 must remain unchanged when the coordinates of any two electrons (both as regards space and spin) are interchanged. This places a restriction of vital importance on the function ψ. Either it must itself remain unchanged or it must change sign.

It is now necessary to distinguish between these two possibilities. The exclusion principle provides the answer. It has been found that if the exclusion principle is to apply to the system as well, the wave function must change sign when any two electrons are interchanged. One consequence of adopting this form of wave function is that the possibility of finding two electrons with the same spin at the same point of space is completely excluded. The importance of this to chemistry will appear presently.

The New Ideas in Chemistry

It was not long before the new ideas were introduced into chemistry. In 1927 Heitler and London made the first application of these novel methods to the structure of a molecule and showed that the theory successfully accounted for the attraction of two hydrogen atoms for each other. It is true that the calculated value of the energy of formation of the hydrogen molecule differed considerably from the observed value, but it was of the right order of magnitude, and it was evident that for the first time here was a promising start towards an understanding of a covalent bond. The method of calculation used by Heitler and London was similar to that already used by Heisenberg in dealing with the structure of another two-electron system, the helium atom. The secret of success lay in constructing a wave function which had the necessary properties to take into account the identity of electrons and the exclusion principle. The wave function in terms of space and spin coordinates was so designed that it changed sign when the two electrons were interchanged. We know now that other wave functions can be devised to have the required properties and that from them also calculations can be made of the energy of formation of the hydrogen molecule. The method used by Heitler and London was not the only one, or indeed the right one, to give a deep understanding of the forces at work in a molecule to produce stability.

One important new idea came out of the Heitler-London approach. Once their method of calculation was generalised by Pauling and others to more complex molecules, it emerged that there were different ways of obtaining a wave function with the correct symmetry properties. Further, it was found that if such wave functions were combined together as linear sums (the symmetry properties thus being preserved), the resulting expression gave a better representation of the system, for when used to calculate the energy it gave lower values and thus indicated greater stability. Now the component parts of the wave functions could each be described (roughly) as typifying a structural formula in the usual chemical sense. Thus emerged the idea that often a molecule cannot be described satisfactorily by any one classical structural formula. It must be regarded as a composite state, best described as a superposition of different structural formulae. This phenomenon was at first called ' resonance,' but the description is misleading for it suggests actual oscillation between several alternative patterns, whereas the true state is intermediate between all of them and simultaneously involves in varying degrees the properties of each. The idea has proved of great value in chemistry in clearing up many phenomena which had previously remained obscure.

One weakness from a theoretical point of view is that the method tacitly assumes that a precise meaning can be attached to each structural formula, and that a single or double bond in different environments has the same significance. Actually it is not possible to describe even the simplest ' single ' bond, viz. the bond in the hydrogen molecule, accurately without having

recourse to the idea of superposition of different conceivable configurations, ionic as well as covalent. Nor does the method make clear why it should be necessary to superimpose different electronic structures. It would be helpful if some understanding could be gained of the influences at work in molecules to produce properties which seem sometimes to correspond to one kind of structure, sometimes to another, and in some cases to be intermediate between those expected of two or more structures.

Molecular Orbitals

It is for this reason that another form of description of molecules has been found to be useful. When several electrons move in the *same* field of force, the wave function of the system can be expressed *accurately* as a product of functions of the coordinates of the individual electrons, or as a sum of such products. Each such function corresponds to a distribution throughout the field of force and is called an *orbital*, because this term implies a generalisation of the notion of an orbit.

It seemed natural therefore to begin with molecules on the assumption that the fields of the electrons on each other could be so smoothed out that all electrons moved in the same field. The problem was then soluble in principle, and though it was necessary in practice to make approximations concerning the form of solutions, this did not detract from the essential soundness of the approach. According to this method the electrons could then be assigned in pairs to a set of functions, each of which represented a definite energy. Each such function depended on the space coordinates of an electron only and for convenience it was called a *molecular orbital*. The energy of the system on this simple view was the sum of the energies of the individual electrons.

It should thus become clear that the term ' molecular orbital,' as first introduced, represented any one of the set of solutions of a wave equation of a single electron in a certain averaged field. The term was thus synonymous with a wave function. The orbitals determined in this way had an important property. If two different ones were multiplied together and the product integrated over the whole of space, the result was equal to zero ; they were said to be ' orthogonal.' This feature has proved to be a necessary one for the application of the exclusion principle. It is as though in molecules, as in music, nature prefers a form of harmony in which the ' notes ' are compatible and the chords are pure.

The method proved to be powerful because the orbitals were found to have characteristic symmetry properties and could be divided into classes. Thus it appeared that if a molecule possessed a plane of symmetry, then some orbitals were symmetric about this plane while others were antisymmetric, that is at corresponding points on either side of the plane the orbital had values equal in magnitude and opposite in sign. One important application of this has been made to conjugated molecules, for they usually have a plane of symmetry through the nuclei.

It should, however, be noted in passing that while the orbitals have these different symmetry properties, the charge distributions appropriate to all the orbitals are symmetric about the plane of symmetry, for they are obtained from the square of the orbital values.

This simple form of molecular orbital theory has been used extensively and has helped to clarify much that was obscure in chemistry. It was used by the writer for diatomic molecules and applied with great success by Hückel to conjugated molecules. It has been developed and applied to various problems of structure by Mulliken, Coulson, Longuet-Higgins, the writer and their co-workers, and by many others. Recently Dewar has made numerous applications of the method and, in particular, has given a simple treatment of the colour of conjugated molecules in terms of their structure.

The idea that an electron may be distributed throughout the whole of a molecule is at first strange to the chemist, who has been brought up to think in terms of localised bonds. But the principles of the wave theory force us to accept this view. When an electron is removed from a molecule by the absorption of light or by other means, it must be regarded as taken from the whole molecule and not from a localised part. The

' hole ' which is left is closely related to the pattern of distribution represented by a molecular orbital.

There has been a further development of the molecular orbital theory. In a real molecule the fields of force acting on different electrons are not exactly the same, because the exclusion principle forces the electrons into different orbitals and the effect on one electron of the repulsive field of all the others varies according to the orbital it occupies. For this reason the wave function cannot be expressed accurately as a product, or a sum of products, of functions of the coordinates of each electron. Progress can be made if in spite of this the same form of wave function is adopted and individual orbitals determined in such a way as to give the ' best ' solution. By this is meant that solution which renders the calculated value of the energy a minimum. This method of calculation automatically introduces a form of averaged field on each electron, but it is different for each. This field is the one that would arise if *all the other electrons* were smeared out in space according to their orbital distribution. Progress in this stage of the theory has been made mainly since 1949 and it has been found that a new range of problems has come within its scope. Applications of the method made at Cambridge and Chicago hold promise that it will be able to deal not only with structure but also with ionisation potentials of molecules and the interrelation of the one with the other.

There is further room for improvement. It will be necessary finally to take into account the difference between the averaged field, even though this is different for each orbital, and the actual field. It is this difference alone which prevents the theory from being accurate in all respects. Thus this method of calculation makes clear that the complexities of the theory arise entirely from the fields of repulsion between electrons, for these depend on the simultaneous position of two electrons and thus involve six coordinates. Successive refinements of the theory represent stages in the process of calculating the effect of one electron on the distribution in space of another. The problem is thus one of finding the effect of electrostatic forces on the spatial correlation of electrons relative to each other.

Some Models of Molecular Systems

These complexities can be avoided and the physical significance of the principles determining molecular structure made clear if attention is first directed to some simple systems in which the particles move in the same field. The process of calculation follows closely that used for molecules by the orbital method.

Model 1. Because it can be used to illustrate some of the properties of butadiene, the first example to be considered is the problem of two particles of the same spin moving in a region of finite length a within which there is no force field. It is supposed that the particles are trapped within this region and that the boundary walls are such that there is no penetration. It is sufficient for a discussion of a problem such as this to concentrate on the behaviour of the particles in the one dimension across the region. To simplify the discussion it will be assumed that the particles do not interact with each other by any direct force field. In this, as in all atomic and molecular problems, it is necessary first to find the appropriate wave representation of the particles. For a single particle moving in such a force-free field the wave function can be any one of a set of *sine* functions, each fitting into the region in such a way that it has zero value at the two extremities. This implies that the half-wave lengths of the functions fit into the width of the region an integral number of times. To each function corresponds a definite energy and the set can be arranged in an increasing sequence, $E_1, E_2, E_3, \ldots$. As the particles have the same spin, the lowest energy of the two is $E_1 + E_2$, because they must occupy *different* orbitals. The wave function of lowest energy is $\sin(\pi x/a)$ and the next lowest $\sin(2\pi x/a)$. These are the two occupied orbitals.

It is necessary now to take into account (i) the identity of the particles, so that each may occupy either orbital with equal probability, and (ii) the exclusion principle. As the particles do not interact with each other directly, the correct wave function is proportional to the difference of $\sin(\pi x_1/a)\sin(2\pi x_2/a)$ and $\sin(2\pi x_1/a)$

sin $(\pi x_2/a,)$ and this is an accurate solution satisfying the necessary conditions.

Once the wave function $\psi(x_1, x_2)$ has been obtained, it is possible to deduce the properties of the system. Thus the square of ψ, multiplied by $dx_1 dx_2$, is interpreted as the probability that one particle is between x_1 and $x_1 + dx_1$ with another at the same time between x_2 and $x_2 + dx_2$.

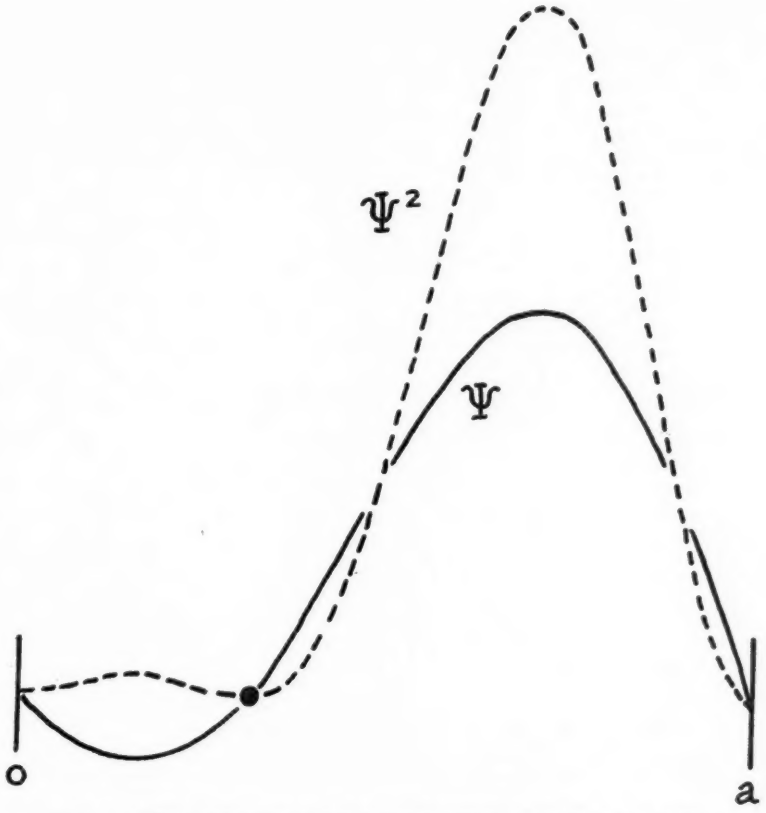


Fig. 1.—The distribution of one particle relative to another due to the exclusion principle.

From this we can derive the probable distribution of one particle relative to the other. In figure 1 is shown the form of the wave function for a given position of one particle and the probability distribution of the second particle relative to the first.

It is evident that the second particle avoids the first and that its most probable position is between the first and the further boundary. It is to be noted that this does not arise from any mutual repulsion of the particles because it has been assumed that none exists. It arises only from the exclusion principle.

As a corollary to this example it is possible to consider the case of four particles moving in the same one-dimensional field, there being two of one kind of spin and two of another. The second pair of particles can also be assigned to the two lowest energy states without conflicting with the exclusion principle. The wave function now consists of products of four sine functions, one for each orbital to which the particles are assigned. In addition it is necessary to introduce factors to indicate the spin to be associated with the particles in each arrangement. The complete wave function is so arranged that it changes sign for any interchange of electrons; it takes the form of a determinant.

For this system it is found that the distribution of one particle relative to another of the same spin is the same as before. One avoids the other. On the other hand, particles of opposite spin can move independently of each other. There is no correlation between them. The probability distribution of one relative to the other has a constant value across the region. This represents a feature which appears in all molecular systems. The exclusion principle does not itself require any correlation between electrons of opposite spin. Whatever correlation exists arises from other causes.

Model 2. A model which can be used to illustrate some of the properties of the benzene molecule is that of six particles moving in a ring of constant potential, three particles each of opposite spins. Again it is supposed that the particles do not interact with each other by force fields.

As in the first example, it is instructive to consider first the case of particles of the same spin. The wave function consists of three factors, each of which is a solution of the wave equation for a single particle moving in the ring. The solution of lowest energy has a constant value round the ring, say C, and the energy is zero. There are two solutions of next highest energy and they are $\sin\varphi$ and $\cos\varphi$, where φ is an angular coordinate determining position. The wave function of the three-particles system is made up of a linear sum of terms such as

$$C(1)\sin\varphi_2\cos\varphi_3,$$

indicating that one electron is assigned to each orbital. When this linear sum is arranged to change sign for any change of electrons, it proves to be proportional to

$$\sin(\varphi_1-\varphi_2)+\sin(\varphi_2-\varphi_3)+\sin(\varphi_3-\varphi_1)$$

and this vanishes whenever two of the coordinates are the same. No two particles ever occupy the same place in the ring simultaneously.

The maximum value of the wave function occurs when the three particles are disposed at equidistant intervals round the ring. Thus the system tends to adopt trigonal symmetry and this property arises solely from the wave representation of the particles and the exclusion principle. The probability distributions of one particle for given positions of the others are shown in figure 2. Again we see the tendency of the particle to take up a position midway between the other two.

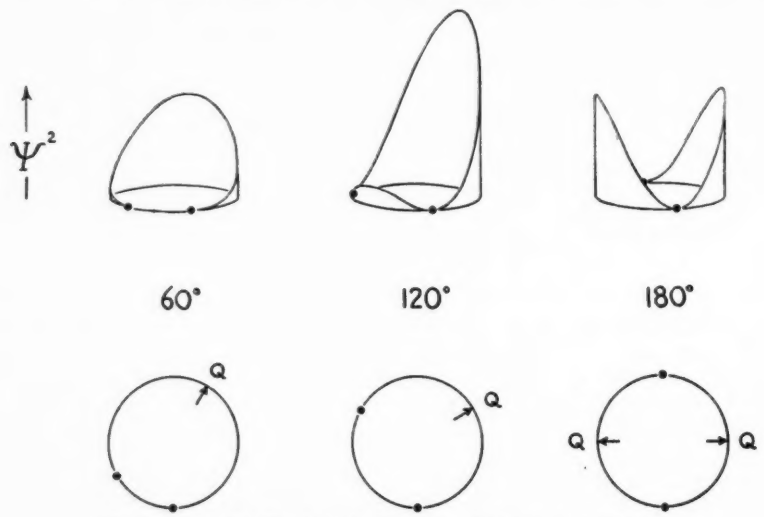


Fig. 2.—The distribution of a particle relative to two others.

When there are three pairs of particles of opposite spin, each set of three of the same spin has the properties just described. In particular each system tends to arrange itself so that the three particles are equidistant round the ring. But there is no correlation between the two systems.

They move round the ring independently of one another.

Model 3. Because of its interest in illustrating some of the properties of carbon and complete octets of electrons, it is profitable to examine in a similar way first a system of four particles, each of the same spin, confined to move on a spherical surface, and then a system of eight particles.

Treatment on similar lines to that given above shows that the four particles of a given kind of spin tend to arrange themselves symmetrically on the sphere so that the lines joining them form a regular tetrahedron. This represents the most probable distribution. Other arrangements can occur, though with less probability.

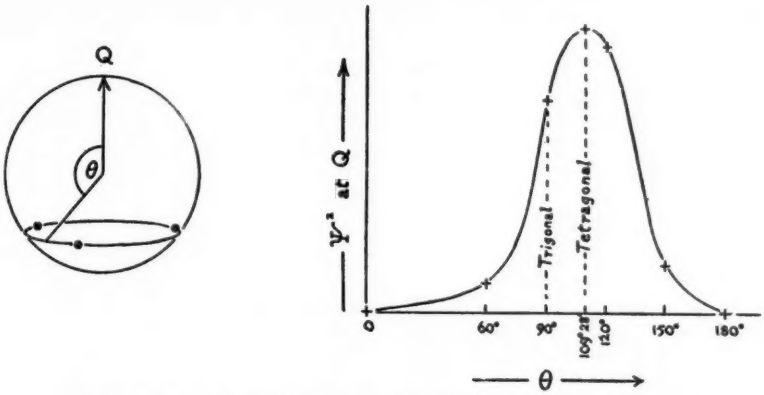


Fig. 3.—The distribution of a particle relative to three others on a spherical surface.

Thus we see in this example the emergence of some of the tetrahedral properties of carbon, for in its tetravalent state it can be regarded as having four electrons somewhat in the way described, though the problem is more complicated because of electrostatic forces.

As in the other example considered above, the two sets of four particles each of the same spin tend to adopt tetrahedral configurations among themselves but the exclusion principle does not cause any correlation between the two sets.

Some correlation is produced by electrostatic forces but it is weak compared with that just described. Calculations show that these forces increase the probability that four electrons of the same spin will take up a tetrahedral arrangement. Correlation between electrons of opposite spin is so slight that a complete octet of electrons may be described as consisting of two loosely coupled tetrahedra. Of course it must be emphasised that while a tetrahedron represents the most probable configuration of each set of four, there is considerable flexibility about that arrangement.

The Physical Significance of the Exclusion Principle

These models bring out a property which holds for all electronic systems, whether they are atoms, molecules or solids: *Electrons of like spin tend to avoid each other.* This effect is most powerful, much more powerful than that of electrostatic forces. It does more to determine the shapes and properties of molecules than any other single factor. It is the exclusion principle which plays the dominant role in chemistry. Its all-pervading influence does not seem hitherto to have been fully realised by chemists, but it is safe to say that ultimately it will be regarded as the most important property to be learned by those concerned with molecular structure.

Some Examples of Molecular Structure

In actual molecules there are three main factors determining structure, viz. (i) the exclusion principle, (ii) the field of the atomic nuclei on each other and on the electrons, and (iii) the forces of the

electrons on each other. In methane, for example, the electrons are attracted to the central carbon nucleus and to the hydrogen nuclei; also the hydrogen nuclei repel one another. Under their own forces alone the hydrogen nuclei would tend to take up a tetrahedral arrangement. The electrons may be regarded as already suitably disposed to receive them and to provide the maximum attraction when the nuclei are most deeply embedded in the electron cloud. In such a system the exclusion principle, the repulsion between the electrons and the repulsion between the nuclei all act in unison to produce the same effect. Whereas in the neon-like structure there is little correlation between the sets of electrons of different spins, in methane both sets are attracted by the hydrogen nuclei, and the two tetrahedra tend to be drawn into coincidence. Thus the eight electrons may be regarded as arranged in pairs in a tetrahedral manner *relative to each other* but constantly interchanging their positions. This is the interpretation to be given to localised bonds.

The structures of molecules such as ammonia, water and hydrogen fluoride are closely related to that of methane. In each there is an octet of electrons. In ammonia the two tetrahedral distributions of electrons are held by the three hydrogen atoms. Relative to the three NH ' bonds ' there is a ' lone pair ' projecting out in a direction approximately at a tetrahedral angle to them. Figure 3 gives some indication of the reason for this. The angles between the bonds and the lone pair differ from tetrahedral ones only because of the forces between the nuclei and the electrons near them.

Similarly the water molecule may be pictured as having two pairs of electrons projecting out in directions nearly at tetrahedral angles to the two OH bonds, and the HF molecule as having three lone pairs of electrons projecting out behind the HF bond. Figure 3 again may be used to illustrate this, the diagram showing equally well the most probable distribution of three electrons of the same spin when the position of the fourth is given.

The great significance to chemistry of these distributions of lone pairs is that they provide electron-rich regions and so are particularly reactive to electrophilic agents. The type of attraction between molecules or groups of molecules known as the hydrogen ' bridge ' may be attributed to the interaction of a positive centre and an electron-rich distribution. These regions also contribute substantially to the electric moments of the molecules and thus have a major effect on their physical properties. Thus the characteristic behaviour of these and similar molecules may to a major extent be traced back ultimately to the operation of the exclusion principle.

Were it not for the exclusion principle all the electrons would crowd into the orbital of lowest energy and this would mean that they would tend to be near the nuclei or in those regions between them where the field is strongest. But the exclusion principle forces electrons into parts of space where they are remote from some of the nuclei. Thus in the molecule of carbon monoxide it is found that the ten outer electrons are to be assigned to five orbitals, of which only one gives a concentration of electron density between the nuclei. In one of the others the region of greatest concentration is near the oxygen nuclei on the side remote from the carbon atom. This is shown in figure 4. The contours show where the orbital has the same value.

In another orbital there is a similar distribution near the carbon nucleus on the side remote from the oxygen atom. These latter distributions each make large contributions to the electric moment of the molecule but they are in opposite directions and nearly cancel out. Though carbon monoxide has a small overall electric moment, there are asymmetries of charge distribution within the molecule which have chemical significance.

Conjugated Molecules

One of the most interesting and profitable applications of the new ideas has been to the structure of conjugated molecules. The nuclei of a conjugated system as a rule lie in a plane. The orbitals occupied by electrons in such a molecule can be divided into two distinct types. In the one the orbital distribution is symmetrical about the nuclear plane; in the other it is antisymmetrical—

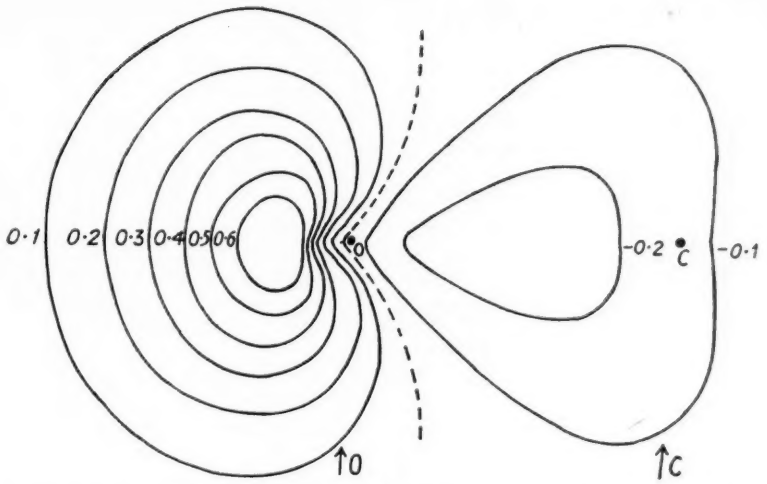


Fig. 4.—An electron distribution in carbon monoxide, showing a concentration of charge outside the nuclei.

that is, the value to be associated with the orbital at corresponding points above and below the plane is the same in magnitude but opposite in sign. It follows that in the latter case the orbital has a zero value at all points in the nuclear plane. It is said to have a nodal plane.

In the language of chemistry the electrons in the single bonds of a conjugated system belong to the symmetrical type, the electrons taking part in conjugation to the other. The former are called σ-electrons, the latter π-electrons. Carbon atoms each provide one electron of the π-type, nitrogen and oxygen atoms sometimes one, sometimes two, according to the other atoms combined with them.

It is a consequence of these different symmetry properties that the π-electrons can for many purposes be dealt with separately. The four π-electrons of butadiene can be dealt with theoretically very much like the four particles in a one-dimensional box, given above. In the normal state of the molecule the electrons consist of two pairs of opposite spin, these being assigned to the two lowest orbitals of π-symmetry. These orbitals have properties along the chain somewhat similar to the sine curves of the model ; in particular, electrons of the same spin tend to avoid each other.

The difference between the model and the molecule lies in the kind of field in which the electrons move. In the molecule the carbon nuclei attract the π-electrons and the saturated electrons repel them. The net effect is that the π-electrons move in a field which changes periodically along the molecule, but they are in fact trapped in a box-like domain. The fluctuations in the field along the box do not alter fundamentally the wave representations of the electrons. There are thus three main factors at work in determining the most probable configuration of the four π-electrons : (1) the nuclei, which attract the electrons, (2) the exclusion principle, which tends to keep electrons of the same spin apart, and (3) the mutual repulsion between electrons whatever their spin.

The attractive field of the carbon atoms will tend to keep one electron near each of them on the average, but the exclusion principle influences the arrangement of spins. If, as has now become usual, spins of one kind be denoted by the symbol α, and those of the opposite kind by β, the combined effect will be to render the arrangements α, β, α, β, and β, α, β, α, more probable than those of the type α, β, β, α, with two neighbouring spins. It may happen that two electrons are simultaneously near one carbon atom, provided the spins are opposite. Such an arrangement might be written αβ, —, α, β with others of a similar kind. This would correspond to what is usually called ' an ionic structure.' The mutual repulsion

of the electrons will, however, reduce the probability of these configurations.

These general features have been confirmed by detailed calculations of the electronic structure of butadiene. Thus, if an electron of given spin is assumed to be at a fixed position near one of the terminal carbon atoms, say C_1, then the probability of finding another of the same spin near the same atom is very small, the probability rises to a maximum on carbon atom C_3 and has a smaller probability of second carbon atom can move towards the first under the influence of the field without conflicting with the exclusion principle. The β-electron on the fourth atom can do likewise, but the α-electron on the third carbon is held off. Thus we can regard the α-system as coupled together and the β-system as coupled together, but with only loose coupling between the two sets due to electrostatic forces.

The process of shift of π-electrons in electric fields is illustrated by figure 5,

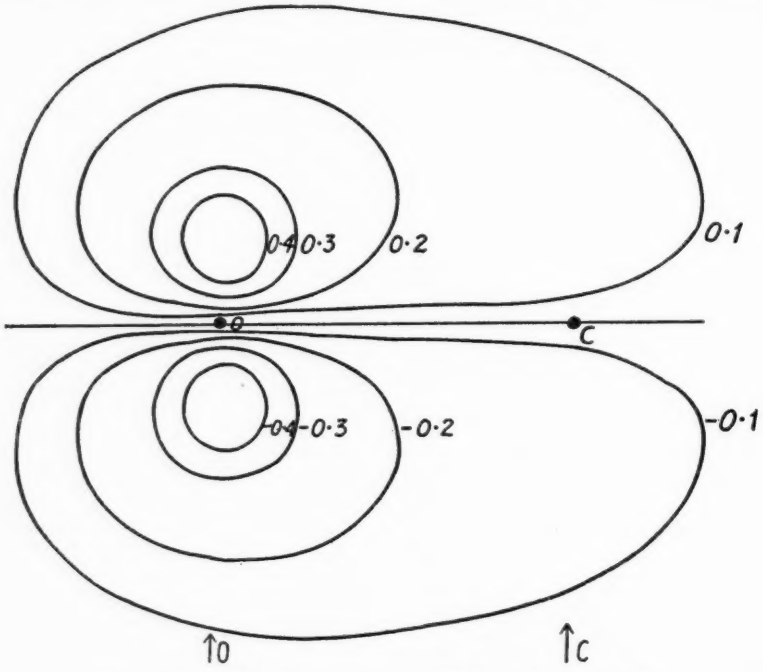


Fig. 5.—The distribution in a π-orbital in carbon monoxide.

being at the other two. On the other hand, the probability of finding an electron of opposite spin has maxima at the second and fourth carbon atoms, C_2 and C_4.

This view of the inner structure of the molecule permits some insight into the processes at work in producing the property of alternating polarity. Suppose that under the influence of a reacting agent of an electrophilic kind the attractive field at a terminal atom is increased. There will then be a tendency of other π-electrons to move towards it; and the α and β electron systems may behave differently. Thus if the sequence of spins is α, β, α, β, then the β-electron from the which shows the distribution of π-electrons in carbon monoxide. The contours show equal values of the orbital, rising to a maximum value above and below the oxygen nucleus. It is probable that there is a similar shift in conjugated systems wherever the —C═O group occurs. This permits a corresponding shift of all electrons, of the same spin on alternate atoms.

A similar picture of the unsaturated electrons in benzene can be drawn on the basis of the model of six electrons moving in a ring, considered earlier. The property of two sets of three electrons of the same spin, each coupled in trigonal symmetry within itself, but moving

independently of each other, is modified by the electrostatic fields of nuclei and electrons. These fields will tend to produce an arrangement of alternating spins round the ring and there are two ways in which this can happen. As in the case of butadiene, an increase of attractive field at any one atom, whether the electron spin be α or β, will tend to cause a shift towards it of the set of electrons of the other kind of spin, that is of the electrons in the ortho and para positions. These positions will thus tend to become deactivated. If, on the other hand, the electron at one carbon is repelled by the field of another group, then the set of which it is a member will move away and cause enrichment at ortho and para positions relative to the meta positions.

The Elusive Electron—Two Points of View

It is not to be assumed that such a picture of benzene, or of butadiene, is sufficient to understand fully how the molecule will behave in all conditions. The properties of electrons in molecules are too elusive for that. We cannot say with certainty that an electron is here or is there. The electron density in a particular part of a molecule may on the average add up to one unit, indicating a charge equal to that of one electron, but the charge density in that region is made of many parts, as though the electron were playing many roles at the same time.

Two analogies may illustrate the dual behaviour of electrons. In some forms of dance, as in Scotch reels and square dances, there are four partners and for most of the time the dancers are to be seen in pairs at corners of the square performing some kind of figure. But there is constant interchange of partners and the identity of the pairs at any particular position does not remain the same. If any one dancer were to fall out, the gap created in the movement would appear first at one corner of a square, then at each of the others in turn. The 'hole' thus left might be regarded as equally divided between the four corners of the square and thus to affect the whole equally. The removal of an electron from a molecule may similarly be looked upon as leaving a 'hole' in the whole system.

We might also compare a molecule with a layer cake. There are two ways of dividing up the cake. It can be taken apart layer by layer, each with its own characteristic flavour. Or it can be cut into segment portions in the normal way. Either method could at each stage give the same fraction of the whole. Electrons removed from a molecule by the absorption of light leave according to the layer method. Those removed by the chemist in a reaction are usually drawn as from a segment portion. But there is one important respect in which the analogy fails. When a portion of a cake is removed, the rest remains as it was. In a molecule this is not so. When an electron is removed by taking away an atom or part of the framework, the rest of the molecule adjusts itself to a new set of layer patterns. These patterns are the molecular orbitals appropriate to the new framework.

Butadiene and Benzene Reconsidered

These properties of electrons may be illustrated by one or two examples. We have already discussed the structures of butadiene and benzene from one point of view. It is instructive to consider them from another. If one of the π-electrons in butadiene could be trapped by some means, say the one on carbon C_1, the other three would distribute themselves among carbons C_2, C_3 and C_4 as though they formed an allyl radical. They would occupy the two orbitals, lowest in the energy scale, with two in the lower one and one in the upper. The upper one, like the upper one in the box model considered earlier, has a node at the centre; that is, the electron in it avoids the centre atom (C_3). Let us next suppose that the trapped electron of butadiene is released and is then strongly repelled along the chain by some nucleophilic agent. In the limiting case when it is forced on to the allyl-like residue, the exclusion principle obliges it to occupy the upper orbital, that is to divide itself equally between carbon atoms 2 and 4. These would then become reactive to electrophilic agents.

In a similar way an electron on a carbon atom in benzene, say on C_1, if forced by some external agency on to the rest of the

molecule, would be obliged to take up an orbital belonging to the pentadienyl residue. A simple, but approximate, calculation of the orbitals of this residue leads to the result that the one which is available, and lowest in the energy scale, has zero values on atoms C_3 and C_5, the meta positions. An electron in this orbital divides itself equally between atoms C_2, C_4, and C_6, the ortho and para positions. While this state of affairs represents an extreme case, it illustrates the tendency of the benzene nucleus to become activated preferentially at the ortho and para positions when an electron is repelled from one carbon atom into the rest of the ring.

The effect of taking electrons out of the ring can be illustrated by considering what happens when the CH_2^+ group is attached to form the benzyl ion. A simple orbital treatment shows that the six electrons redistribute themselves among the seven centres in such a way that the ortho and para positions are deactivated, the meta positions to a first approximation being unchanged.

The Present Position and the Future

It has not been the purpose of this Address to detail the methods of calculation which have been developed to deal with molecular structure but rather to discuss their physical content. An understanding of the physical ideas involved may help the experimenter to appreciate the processes at work in determining the way chemical reactions take place and the theorist to see the significance of the difficulties which arise in his calculations. There has been remarkable progress in the application of the wave theory to the structure of molecules, and the advances made have inspired general confidence that the principles governing the behaviour of molecules are now known and that what is required is a rigorous method of calculation in accordance with these principles. It appears from what has been said above that the main difficulty in the theory is in the calculation of the effect of the repulsive fields of the electrons on each other. Other aspects of the theory do not present serious obstacles to calculation. It is because of this outstanding mathematical difficulty that the successes of the theory up to date have been mainly of a qualitative kind. Calculated values of observable quantities are as a rule approximately the same as those determined by experiment, but the stage has not been reached when computation can take the place of practical determination.

None the less, the ramifications of the theory have been far-reaching. The covalent bond, which for so long was a mystery, is now understood. The gradations between covalency and electrovalency have become clear. The factors determining the shapes of molecules have become more precise. The properties of conjugated molecules have been found explicable in terms of the same principles which determine the structures of other systems. A further success of the theory has been in bringing into close relation to each other the chemical and spectroscopic properties of molecules. It is now possible to understand the conditions under which molecules may show colour and promising progress has been made in advancing a theory of dyes. Much remains to be done but the prospect of further advance is bright.

In justification of the theme of this Address some words written by Faraday to Clerk Maxwell may in conclusion be quoted. 'When a mathematician engaged in investigating physical actions and results has arrived at his own conclusions, may they not be expressed in common language as fully, clearly and definitely as in mathematical formulae? If so, would it not be a great boon to express them so—translating them out of their hieroglyphics that we might work upon them by experiment?' It may not be possible for the theorist of today to attain an ideal so clearly set forth, but the task remains clear and insistent, now as then, that the attempt be made to express as simply and clearly as possible the physical meaning of the mathematical theories that have swept through the whole of chemistry.[1]

[1] I am indebted to Dr. S. C. Nyburg for working out and drawing figures 1, 2 and 3, to Dr. R. C. Sahni and the Faraday Society for permission to use figures 4 and 5, and to Dr. P. P. Manning for allowing me to refer to his unpublished calculations on butadiene. I am also grateful to Prof. H. D. Springall, Prof. I. N. Sneddon and Dr. S. C. Nyburg for reading this address in proof and for their valuable suggestions.

THE RELATIONSHIP OF PALAEONTOLOGY TO STRATIGRAPHY

Address by

DR. C. J. STUBBLEFIELD, F.R.S.

PRESIDENT OF SECTION C

WE meet this year in a city which is a shrine of British palaeontology, for from Oxford came the first printed pictures of any British fossils. These appeared in *The Natural History of Oxfordshire*, published in 1677, from the work of the first Curator of the Ashmolean Museum, Robert Plot. It is well known that Plot was as mystified as many of his contemporaries and successors concerning the real nature of fossils, though in several cases he made accurate comparisons with their modern analogues. Nevertheless, he performed a service in indicating Oxfordshire localities for the finding of the 'formed stones' which he illustrated and he offered a basis to others for comparison with their finds made elsewhere. It is interesting to recall also that Plot's immediate successor as Curator of the Ashmolean Museum, Edward Lhuyd, also acquired fame from the publication in 1699 of the first book entirely devoted to British fossils, though, like Plot, he misunderstood their nature.

Oxfordshire has other claims for pilgrimage. It contains the birthplace of William Smith, a man whose discoveries gave new aid to the geological approach to stratigraphy. Furthermore, the richly fossiliferous Jurassic rocks of which this county is composed have been the subject of particularly detailed study, here, in other counties and in other countries. In these studies, palaeontology has been the key to many basic principles of stratigraphy; to the division and subdivision of rock-successions, to the recognition of non-sequences and pauses in deposition, to current concepts of facies, changes and of cycles of sedimentation and to palaeogeography.

Both palaeontology and stratigraphy are observational rather than experimental sciences, nevertheless however accurately the observations are recorded, they are but retrospective in so far as they relate to natural processes. Whereas the biologist and geographer can see most of the natural processes in operation, the geologist observes the results, and these, in places, have had superimposed upon them subsequent complicated moves of nature. He is thus left to deduce the various mechanisms and the tempo of these processes, using as comparison phenomena in operation today. Before adequate appreciation of the relationship of present to past events could be attained, certain elementary facts required establishing and a number of false suppositions had to be eliminated.

It is commonplace knowledge to us today that the geological record is incomplete but our predecessors had to discover this by empirical methods and the study of fossils contributed conspicuously to this knowledge. This is an essential perspective but there is another view-point. Contemporary palaeontologists and stratigraphers can contemplate with pride the vast amount of work done by their predecessors, despite the fragmentation of the record; they can also appreciate with optimism the vastly greater quantity of profitable work which awaits them and their successors.

It is my purpose to present a brief historical review and commentary on the development of the relationship of palaeontology to stratal division and correlation.

The name Palaeontology dates from 1834, in which year it was used both by de Blainville and Fischer von Waldheim.

It gradually displaced the older terms Oryctology, Petrefaktenkunde and Petrefaktologie. Palaeontology is the study, in the field and in the laboratory, of the remains of organic life preserved in rocks. Its broad biological objective is to obtain and assess the maximum amount of information concerning every form of life found fossilised, its morphology and growth history, its status in classification, its probable conditions of life, its associates and its range in space and time. Palaeontology proceeds from detailed local studies of fossil faunas and floras towards syntheses which indicate their partial distribution and migration over broad regions ; and to studies which indicate evolutionary change in time.

The aims of stratigraphy are to determine the history of the earth's crust by studying the nature and succession of the sedimentary rocks, their origin, their spatial distribution and their structural disposition. A fossil is an integral part of the rock in which it is found and palaeontology and stratigraphy cannot be divorced ; as sciences dissociated from each other, each is inadequate. Past histories of palpably false phylogenies, presumed from laboratory studies taking little or no account of stratigraphy, are unfortunately not rare.

Many years ago Lyell stated that the fossils present in a particular deposit were a ' function of three variables ': (1) the geological period at which the rock was formed, (2) the zoological or botanical province in which the locality was situated and (3) the physical conditions prevalent at the time, e.g. land, river or sea ; if the last-depth, salinity, turbidity, climate, character of sea bottom, etc. One might add a fourth category, (4) post-depositional events ; dolomitisation for instance usually obscures any small fossils and on occasions so obscures fine features of external patterning of the others as to render specific identification impossible ; pene-contemporaneous silicification on the other hand, may in certain circumstances, as in Middle Ordovician deposits of Virginia, contribute towards perfect preservation of trilobite and ostracod skeletons (Whittington and Evitt, 1954).

In any consideration of the relationship of palaeontology to stratigraphy it is axiomatic that the best results will accrue where the basic data are reliable. The stratigrapher aims to produce a geological map with the strata accurately correlated and the palaeontologist works to ensure accurately determined fossils and knowledge concerning their distribution.

The Confluence of Studies of Fossils with Studies of Stratified Rocks

The organic nature of fossils took a long while to become generally realised. When the word ' fossilia ' was coined by Agricola in 1546, he used it to include almost ' anything dug up.' It was not until Lamarck's (1802) redefinition of the word restricting it to organic remains, that minerals were entirely eliminated from the concept. A limited number of fossils was known to Linnaeus when he was preparing his *Systema Naturae*, but geologists had found no compelling reason to collect them. To many biologists who believed in their organic nature they were either organisms destroyed by the flood or remains of animals and plants expected to be found in other parts of the present-day world when exploration reached those parts. The age of the world was thought to be but 6,000 years or so, therefore there was no problem of a long history of past life, in fact no reason for palaeontology. Similar types of argument made the study of stratigraphy no great necessity. Here the philosophies of the Neptunists were added to those of the Diluvialists, long after Steno (1669) had announced the Law of Superposition from his observations concerning the strata of Tuscany. Experience showed later, that a provision to cover the exceptions caused by overfolding and overthrust faulting had to be added to the law. The early lack of this provision impeded the interpretation of the rock sequences in the Scottish North-West Highlands and elsewhere. Sollas (1905, p. 219) reminds us that Steno was also aware of the nature of unconformities and of the organic origin of fossils.

Lyell, Geikie and Zittel are amongst the historians of stratigraphy who have indicated the debt which the subject owes to Lehmann (1756) and later to Füchsel (1762) for their respective studies of the Permian and Triassic strata of Thüringia.

Both these authors gave vertical sections indicating the order of the strata and Füchsel provided a map showing the disposition of the outcrops. From Füchsel's work arose the terms Stratum, Series and Formation. He also gave palaeontological value to some of his formations for he recognised that the Zechstein with its gryphites and that the Muschelkalk with its ammonites were sea-deposited whereas coal associated with land plants indicated the near occurrence of land. His publication ' though it was unfortunately but little known during its author's lifetime, became practically the model for the Wernerian School of geologists, and, more than any other individual work, laid the foundation of that rapid development of stratigraphical geology which began in Germany in the next generation ' (Zittel, 1901, p. 37). Nevertheless, when the Wernerians advocated the doctrine of formation-suites, fossils were scarcely called upon to assist in the identification of the rocks ; the theory of a universal and gradually subsiding ocean, relied on minerals and fragmented rocks for the interpretation of the sequence of events.

Following the unheeded work of Giraud-Soulavie (1780) who divided the strata of Vivarais in the south of France into five divisions on the basis of their fossils, William Smith, working independently in Britain announced the much quoted relationship between ' each stratum ' and its ' contained organised fossils peculiar to itself ' (J. Phillips, 1844, p. 15). The crowning effort of Smith's work was his production of a geological map of England and Wales in 1815. Smith has come to be known as the ' Father of Historical Geology ' (Zittel, 1901, p. 48). With no great palaeontological learning himself, he had convinced his contemporaries of the importance of palaeontology to stratigraphy.

The word Stratigraphy did not come into use until 1865 and it probably took origin from W. Smith's 1817 publication, *A Stratigraphical System of Organised Fossils*. When Smith's manuscript table of strata exposed in Britain between the Coal Measures and the Chalk was circulated in 1799, the science of palaeontology was very young. Stratigraphers, however, paid increasing attention to collecting fossils.

Fortunately, about this time, three distinguished French biologists, Lamarck, Cuvier, and Alexandre Brongniart became interested in geology. Lamarck described various fossil mollusca from strata exposed near Paris. Cuvier and Brongniart examined these strata on the lines of William Smith's work and produced a geological map. They (Cuvier and Brongniart, 1808) found that in a basin floored by Chalk there were successive sets of strata some of marine and others of freshwater origin, and from some of the latter beds Cuvier described the vertebrate fossils. The authors noticed, in tracing the succession upwards through the Calcaire Grossier, that the mollusca became progressively more like living forms. When Tertiary strata from other areas subsequently came under study, observations of this kind were developed by Bronn (1831), Deshayes (1831) and Lyell (1833) to produce a classification of the Tertiary formations on the basis of progressive increase of living species in successively younger rocks. This bringing together in classification of stratal units exposed in several areas, in none of which was the succession complete, resulted from the amalgamation of palaeontological deduction with the application of the Law of Superposition. Neither the evolutionary doctrines of Lamarck nor the cataclysmic theory of Cuvier and his followers impeded the building of a scheme of stratal successions. Subsequently Cuvier from his important studies of vertebrate fossils came to be looked upon as the ' Founder of Vertebrate Palaeontology ' (Geikie, 1905, p. 349) and Lamarck was given an analogous title for his work on the invertebrate branch of the subject. W. T. Gordon (1934) has singled out yet a third Frenchman, Adolphe Brongniart, the son of Alexandre, as the founder of Palaeobotany by his classification of fossil plants which appeared in 1822.

In the various branches of palaeontology, work has proceeded with substantial assistance from biologists. Botanists have consistently been interested in Palaeobotany recognising it as a significant part of Botany. In Palaeozoology, with notable exceptions, zoologists have made their greatest contributions to vertebrate palaeontology.

During the first decades of the nineteenth century, the description and illustration of fossils species gained impetus, mainly using the Linnean binominal nomenclature to assist international understanding of the results. Stratigraphers, also, as they mapped the various rock units in the individual countries became increasingly aware of the desirability of internationally valid nomenclature when formations came to be correlated.

In the following pages are discussed the various orders of division into which the stratal record has been divided ; but it must be remembered that whatever views were held at the time when particular divisions were erected, earth history and the history of organisms are nowadays considered to be continuous processes. Despite the stratal gaps the divisions which we use are employed only as headings for descriptive purposes. The pattern of division has been produced by processes of analysis and synthesis making increased use of palaeontology to classify progressively smaller units of rock thicknesses.

The Eras

The publication in 1822 of Conybeare and W. Phillips' *Outlines of the Geology of England and Wales*, provides a landmark of stratigraphical knowledge of British rocks at that time. Amongst several interesting features we find (p. 166) that the 'Oolite Series' was divided into Lower, Middle and Upper Oolite 'Systems,' each 'system' having as its first member a clay passing up into a calcareous sand series which in turn passes into oolites ; attention was drawn to these sequences or cycles but it was left to later workers to relate them to minor earth movements. These authors further stated that 'each of these beds is characterised by its peculiar organic remains' but (pp. 60–61) 'several of the species, and a majority of genera, are common' to the beds between the Chalk and the Oolitic Series and to the Chalk ; though 'if we compare this suite, either with the more recent shelly beds above the Chalk, or the older associated with the coal and transition series, a marked and striking difference is instantly perceived.'

This last comparison subsequently germinated into the tripartite classification of fossiliferous strata which has survived to the present time, though several authors have pointed out that it lacks fundamental significance (e.g. Rastall, 1944). The three terms Palaeozoic, Mesozoic and Cainozoic were applied by J. Phillips (1841, p. 160) definitely to strata classified on 'the evidence of organic remains which clashes very little with the ordinary scheme founded on structural, mineral and chemical analogies of the rocks.' Phillips admitted his scheme to be founded on contemporary knowledge of European geology and therefore subject to subsequent modification. It is noteworthy that Phillips recognised transitions from group to group and gave no indications that he had been influenced by thoughts of extensive earth movements in determining the boundaries of his divisions. The upper limit of the Palaeozoic was placed between the Magnesian Limestone and the 'New Red formation,' that is at a higher level than that advocated for Palaeozoic by Sedgwick in 1838 when he first used the term. The boundary between the Mesozoic and the Cainozoic was drawn at the top of the 'Cretaceous System' and Phillips listed invertebrate and vertebrate animals as well as plant groups typical of his three divisions. Nowadays Phillips' groups give their names to eras and the Palaeozoic is interpreted as starting with early Cambrian time. Though estimates of the duration of these eras expressed as years have been offered on various bases, one using calculated rates of evolution of vertebrate phyla (Matthew, 1914) and the other of faunas (Lyell, 1867), neither stratigraphy nor palaeontology has produced sufficiently convincing data to gain acceptance of these eras as absolute measures of time in terms of years. Without the recently deduced data from radiogenic isotopes, these 'eras' remain no more highly rated than serial successions and it is salutary to remember that work on radiogenic isotopes indicates that palaeontology has so far contributed serial successions only in the last quarter or less of the history of the earth's crust. Nevertheless, it is important to both stratigraphy and palaeontology to learn that reliable evidence for the duration of the three eras shows that the Palaeozoic lasted 300 $\pm$ million, the Mesozoic 130 $\pm$ million and the Cainozoic

70 ± million years, but as Knopf (1949, p. 6) affirms, the determination of the absolute time lengths of the geological periods still remains a future task.

Whatever its shortcomings, for example, the fact that the apparent changes in the major developments in fossil plant evolution do not coincide with the boundaries used for animal life (Schindewolf, 1950a, p. 27), this tripartite classification continues to serve useful purposes.

The Systems

Having indicated the development of the status of the first order into which the stratigraphical record has been divided, some attention will be given to the second order, namely, the systems, or in time phraseology, the periods. The task of marshalling the various rock groups into the units now termed systems was effected partly before the larger grouping and mainly before the introduction of the palaeontological subdivisions known as stages and zones. The number of systems (excluding Pre-Cambrian) now accepted varies from ten to twelve. They were defined gradually and on variable and mostly empirical bases. Though some degree of palaeontological unity in character within a system was sought, the dividing lines in general were taken at such major physical breaks, or changes in bulk lithology, as seemed to have regional significance. A principle involved in this choice was undoubtedly a belief that the effects of particular earth movements had world-wide significance. Whereas the traditionally recognised systems were established chiefly in continental shelf areas of the northern hemisphere and have been found generally recognisable whenever encountered elsewhere in the world, there are difficulties in tracing systemic boundaries in land-deposited rocks in the southern hemisphere ; problems also arise away from the type areas where marine deposition seems to have been continuous across the systemic boundaries.

Palaeontology played a major and fortunate part in the establishment, by Murchison and Sedgwick in 1840, of the Devonian or Old Red Sandstone System. Murchison had previously erected the Silurian System in 1835 and in 1839 he described its divisions and, with his colleagues, the faunas of those divisions ; furthermore, he had separated the terrestrially deposited Old Red Sandstone from the base of W. Phillips and Conybeare's Carboniferous System. With Sedgwick, he had interpreted the sequence of rocks exposed in Devon. Since marine faunas found in the Devon limestones were considered by Lonsdale to be biologically intermediate between those of the Silurian and of the Carboniferous, the Devonian System was founded ; and since the Old Red Sandstone was known to occur between the Silurian and the Carboniferous in the country further north, the two formations were inferred to be contemporaneous. This correlation of considerable thicknesses of rock of such different origin and lithology was a wider application of the facies researches which Gressly had lately made known in the Mesozoic rocks of the Solothurn Jura and Prévost in the Paris basin Tertiary.

The story of the founding of the Ordovician System by Lapworth in 1879 also shows considerable palaeontological influence. Barrande published in 1852, the first volume of his beautifully illustrated series of monographs on the Système Silurien de Bohême ; this dealt with trilobites and showed that in Bohemia the earlier Palaeozoic rocks held three successive major faunas named, respectively, Primordial, First and Second faunas. The Primordial fauna had not then been recognised in Britain, though by following up a lead given by Barrande, subsequently, it was found by Salter to characterise the [Middle] Cambrian of South Wales. Barrande's first and second faunas, however, were compared with those developed in Murchison's Silurian System. Although the junction between the rocks containing these first two major faunas had been disputed in Britain for some years, the terms Lower and Upper Silurian were in existence and they had attained subsystemic rank. However, the British rocks containing the first fauna were also claimed by Sedgwick as Cambrian. Lapworth in an attempt to resolve the difficulty named these rocks Ordovician choosing the base of the new system at a line below the rocks containing the first fauna, that is, at the unconformity accompanied by lithological and faunal change at the base

of the Arenig strata. The summit, though 'known to be stratigraphically concordant nearly all over the world,' he chose at the base of the Llandovery where 'there is sometimes an actual physical break' (Lapworth, 1879, p. 6). It seems that this last sentence has not yet completely converted French and some southern European geologists to the view that the Ordovician is worthy of systemic separation from the Silurian; nevertheless, the name Ordovician now has almost world-wide currency. There remain, however, several other discrepancies in usage of systemic names as for instance, in the value given to the Permian; and there is varying practice in the use of Tertiary. Here there are four variants. The first is that the Tertiary constitutes a system and this usage, which has much to commend it, was recommended by the VIIIth International Geological Congress held at Paris in 1900. The second is that the Tertiary comprises two systems, the Palaeogene or Nummulitic, and the Neogene and this usage is followed by some European and American writers; the third variant is to regard Eocene, Miocene, Oligocene and Pliocene as being of systemic rank; with the fourth, Tertiary is used as an era name and the Eocene, Oligocene, etc. are called strata. Though the fourth is a minority usage, like the others it has British supporters; this lack of uniformity in British practice is a matter which seems to require discussion.

When strata come under study, approximating in age to the boundary lines between the various generally recognised systems, there have been and will continue to be disputes as to which system a particular stratum should be referred; similar trouble arises as intervening strata are discovered which are unfamiliar in the type areas. Jukes-Browne (1912, p. 10) stated that systems must have 'a palaeontological value' and elsewhere (p. 8) that 'we must always be prepared for the possible intercalation of another system between any two groups that are divided by a decided unconformity.' However, this last course has not since been adopted; Ulrich's 1911 attempts made largely on these bases to introduce Ozarkian and Canadian systems between the Ordovician and Silurian were unsuccessful; and Grabau's suggestion to redivide the Palaeozoic into 13 systems on the 'pulsation theory' of successive age transgressions and regressions of the sea has not been supported.

The Series

Most of the systems have been divided into series, the third order, with epochs as the accepted time units though little uniformity has yet been achieved in nomenclatorial pattern. In systems such as the Devonian or Old Red Sandstone it is sometimes necessary to have separate series connotations according to the differing facies of deposition, or for different organic provinces; efforts are being made, however, in regions where facies interdigitate, to establish age comparisons. For world correlation purposes a simple scheme dividing each system on a palaeontological basis into Lower, Middle and Upper, has much to commend it where three-fold division is applicable. There is, of course, no guarantee that such divisions would be of equal size in any system. The recommendation of the VIIIth International Geological Congress to use as prefixes Eo-, Meso- and Neo- to the systemic name is in fact followed by certain French, German and Polish geologists working among Devonian rocks. The usage of Lower, Middle and Upper is customary practice nowadays when speaking of Cambrian and Devonian rocks but in certain European countries the Carboniferous and Cretaceous are usually divided into Lower and Upper. In the cases mentioned, palaeontological criteria are employed in the division. World-wide correlation value is usually given to the four divisions of the Tertiary System and Paleocene has lately been added to the suite of series names. Nevertheless, in the Palaeozoic systems at least, there are tendencies to retain the traditional series names such as Tremadoc, Niagara and Zechstein and to employ these using faunal data for regional and sometimes inter-continental correlation.

The Stages

When we come to the fourth order divisions, the stages, we use a word which meant to the early French geologists strata occurring in steps, but to which Alcide d'Orbigny gave a much wider meaning. He was a man of considerable geological

Errata. Section C Address, p. 154, left-hand column.

line 54. *For* Ordovician *read* Cambrian.
line 55. *For* Silurian *read* Ordovician.

and biological experience and he had two meritorious ambitions. The first was to produce a universal stratigraphy based on palaeontological chronology. The second was to describe all the fossils found in France belonging to the Mollusca, Brachiopoda, Echinoderma, Bryzoa and Corals, since he believed the occurrence of these to have significance in stratigraphy (Zittel, 1901, p. 506). In this last, he achieved some success as far as Jurassic and Cretaceous species are concerned ; in the first, again his results have been effective in the same two systems, though parts of his philosophy are discredited. The stage, to d'Orbigny, was a division of a terrain or system and each of the twenty-seven stages into which he divided the fossiliferous stratified rocks was considered to have world-wide occurrence and to be characterised by a particular fauna or flora. Being a liberal interpreter of Cuvier's cataclysmic theory, he believed that following the destruction of the organic world at the close of each successive stage, the earth was repeatedly repopulated by its plants and animals (d'Orbigny, 1851, p. 251). This proposition is difficult to assimilate in conjunction with d'Orbigny's belief in lithological facies-changes, for it allows no organic continuity across stage boundaries. Nevertheless, he claimed to name each stage after a type locality where it was best developed, and he relied in correlation, not on lithology, but ' only on the identity of composition of the faunas or on the extinction of genera or families' (d'Orbigny, 1842, p. 9). In discussing the ten stages into which he divided the Jurassic System he referred to the many species characteristic of each stage and those few which passed from one to another he looked upon as remanié derivatives (d'Orbigny, 1850, pp. 600–23) ; he also indicated various localities in which the stage was developed in different lithologies. Though it would be misleading to state that present-day interpretation of the stage concept is completely divorced from palaeontological significance, it has diverged from d'Orbigny's criteria. The cataclysmic bias has been much reduced and the concept may now be defined as a thickness of rock deposited during the time taken to form a known suite of sediments characterised by a particular bulk fauna at the type locality of the stage. The boundaries of the stage at that type section, normally coincide with those of a lithological unit or of a palaeontological zone. It has been proposed that an international body be given powers to interpret previously agreed rules of procedure in disputed cases (Arkell, 1946). The term zone, as introduced and used by d'Orbigny was offered by him as a palaeontological second name for a stage ; subsequent workers gave to zone a more restricted significance and most of the stages now recognised comprise a number of such zones. From the above definition it follows that a stage may, in certain areas, be represented by rocks barren of fossils and the authenticity of its presence may rest on various degrees of subjective reasoning ; on the other hand, when a sequence of distinctive assemblages of fossils is treated as a unit of comparison, general correlation may rest on firmer ground. It has been claimed in the Jurassic System, for stages largely based on ammonite faunas, that whereas separate zonal schemes are desirable for each faunal province, ' stages provide the best medium for correlations of continental scope and for an eventual global time-rock standard ' (Arkell, 1946, p. 30). Though the 1900 Paris International Geological Congress considered that stages had but regional value and were not essential in international correlation, experience has shown otherwise in certain cases. The ammonite-based stages of the Jurassic have already been mentioned ; d'Orbigny's Cretaceous stage names are also still in use with but slight modification and addition, and Glaessner (1945, p. 206) states that the Upper Cretaceous stages (Cenomanian, Turonian, Senonian and Danian) have world-wide recognition on the basis of distinctive assemblages of smaller foraminifera. D'Orbigny's axiom that each stage should be named after a locality rather than a rock type or fossil, has been usually accepted and two lapses he made in his Jurassic scheme have been corrected, for instance, Liasien is now replaced by Oppel's term Pliensbachian. The time-intervals or ' ages ' taken to mark the deposition of a stage, following Buckman's (1898) usage, have received names derived from fossil genera, e.g. Bathonian Stage, Parkinsonian Age.

Considering the successive cycles of deposition indicated by Arkell (1933, p. 54) for the Jurassic System in the south of England, it is possible to relate most of them to the succession of stages, thus supporting the suggestion made by Gignoux (1950, pp. 23–6) concerning the relationship of sea-regressions and transgressions brought about by epeirogenic movements to the thought underlying d'Orbigny's ' Successive creations ' ; for these movements are likely to have redistributed the facies-belts and induced changed faunas into the neritic areas.

Stage nomenclature is much used in the literature of the Jurassic, Cretaceous and Tertiary systems ; palaeontological criteria for definition of these stages in their type localities are variable. Studies on one group of organisms in a given area do not necessarily produce the same boundaries in division as others ; as an example may be quoted the Urgonian which has been shown to pass laterally into Barremian and Lower Aptian. The same difficulty has been encountered with systemic boundaries (Schindewolf, 1928). Furthermore, in different regions stages must be erected from such fossil groups as are there available. Thus, in the Tertiary of various parts of North America, stages have been formed here on vertebrata, there on mollusca and elsewhere on foraminifera. In Palaeozoic literature, stage nomenclature has had a limited application though usage is increasing. The post-d'Orbigny Lower Carboniferous stage names, Tournaisian and Viséan, are widely recognised and in Britain, other Carboniferous stages based on goniatite faunas are coming into use. D'Orbigny, however, by naming his first and second stages respectively Silurian and Devonian, had cast the Lower Palaeozoic stages into so large a mould that replacement-names in Norway by Kjerulf and in Bohemia by Barrande were mostly of systemic rank or almost so. In Scandinavia, the stages moreover carried generic names of fossils in their nomenclature. Nevertheless, in both regions the stages were subdivided on the basis of trilobite faunas. In Britain, Belt's (1867) tripartite division of the North Welsh Upper Cambrian, underlying the Tremadoc Series, had faunal characters given to his Maentwrog, Ffestiniog and Dolgelley beds, and these divisions have been called stages by W. W. Watts in *The Handbook of the Geology of Great Britain* published in 1929. O. T. Jones' (1928) praiseworthy lead in breaking some of the supposedly long-ranged Ordovician and Silurian brachiopods into new species with restricted ranges was followed by Bancroft who also tackled the Trinucleidae on similar lines. This resulted in Bancroft (1933, 1945) formulating a scheme of eleven stages for post-Llandeilo Ordovician strata in Britain. The scheme follows the d'Orbigny method even to the -ian ending of the type-locality based stage names. It is premature to say whether these Ordovician stages will be recognised far beyond the bounds of the organic province of their foundation, nevertheless, they are useful in indicating the distribution of strata with similar faunas.

The Zones

A commonly accepted definition of the fifth order division is that ' zones are belts of strata each of which is characterised by an assemblage of organic remains of which one abundant and characteristic form is chosen as index' (Marr, 1898, p. 68). Not all authorities, however, accept a zone as indicating strata related to time, to some it connotes only relationship to life for ' its limits can go no further than the physical occurrence of a certain fossil or fossils ' (Hedberg, 1954). Others (Spath, 1931 ; Schindewolf, 1950 ; Wedekind) consider a zone to have time but no direct spatial value. Despite these objections, zones continue to be employed as time-stratigraphic units in the region of their development with the knowledge that there will be different zonal tables in each organic province.

Oppel's classical scheme for the Swabian Jura, with two-thirds of the zonal indices chosen as ammonite species, was established by bed-by-bed collecting in a region of gently dipping strata. He sought the vertical range of each species in many districts and made an ideal profile in which he claimed that the divisions of the same age were always characterised by the same species (Oppel, 1856, p. 3). It is usually accepted that Oppel's zones were faunizones in the sense used by Buckman (1902, p. 557) that is, they are based on

the vertical and lateral range of an assemblage. It is relevant to recall that Arkell (1933, p. 50) has drawn attention to the work of German geologists in relating minor cyclic earth movements to some of the Jurassic faunizonal boundaries.

The merits of ammonites as zone fossils are undisputed ; the intense evolutionary activity of the group with the wide distribution of apparently short-ranged species has contributed to the success of zonal schemes in various systems. Similar praise may be accorded to goniatite-, graptolite- and certain foraminiferal schemes. These schemes have had considerable bearing on the interpretation of sequence and structure in certain areas. As when larger stratigraphical units are missing from a sequence, so missing zones may show the scale of stratal gaps. It is probably by using sequences of zones rather than individual zones that the zonal method has had most bearing on general stratigraphy. Instances are frequent where the method has been invoked with apparent success to indicate minor folding and faulting ; an early example was Hébert's work (1873) using the vertical distribution of named species of ammonites, belemnites and lamellibranchs in the Chalk of the Paris basin. Lapworth undertook the description and classification of graptolites to erect a zonal scheme from them to try to explain the complicated disposition of the rocks in the Moffat district and was thus led to an interpretation of isoclinal folding and faulting (Lapworth, 1878). Similar but smaller scale success came to Cobbold (1927) from trilobite zones in the Cambrian of Comley. The method has also been successfully used to correlate rocks in different facies by Lapworth with graptolites, and by Bisat with goniatites.

Since the very existence of zonal boundaries is in many cases associated with changing strand-lines or courses of migration, it is not surprising that lineages built on evolutionary principles can rarely be traced. Exceptions occur in rocks where deposition has continued with little interruption across these boundaries, and as an example may be cited the evolutionary work by Rowe on the various species of *Micraster* in the Upper Chalk (1899) ; Bisat's mutations of the goniatite *Reticuloceras reticulatum*, since they are found in shale bands following successive grits, presumably evolved elsewhere and periodically became introduced into their present area as suitable environmental conditions allowed.

It is well known that some groups of organisms are good zone fossils whereas others either because of sedentary habits or their apparently long-ranged species are unsuitable. On the other hand, we are sometimes reminded (e.g. Berry, 1941, p. 164), that it is ' a general principle and it is worth emphasis that no group of organisms is valueless in chronology or paleobiology, given students who have knowledge to interpret their observations.' It has not infrequently transpired in palaeontology that the ' dross ' of one generation has been a source of value to the next. One may think of the graptolites already mentioned. Another case based on more unpromising fossils is the Coal Measures non-marine lamellibranch zonal scheme evolved by Trueman and his colleagues, which has had far more than local success in correlation, particularly when integrated with other faunal and floral schemes, and with lithological successions.

The very considerable application of foraminifera to zonal work requires some reference. Foraminiferal zonal schemes based on species of larger foraminifera have been in use in Cretaceous and Tertiary stratigraphy for some years but recent intensive study by oilfield micropalaeontologists has brought into use schemes based on assemblages of small foraminifera. These schemes have produced remarkable results in local correlation problems and are now being extended to regional correlation. Reference to the extensive literature of this work is given by Glaessner (1945). For English Chalk, Williams-Mitchell (1948) followed by T. Barnard and F. T. Banner have prepared foraminiferal range data complementary to the zonal schemes based on mixed macrofaunal assemblages.

The palaeontological evidence for correlation of one deposit with another has varying value in different cases. The species always require accurate determination and the evidence may need careful assessment in relation to conflicting non-palaeontological evidence. Though every fauna and flora is

restricted to certain ranges of lithology—some such as reef-building organisms are very closely restricted. On occasions, some of the organisms can be utilised for zoning with limited success as for instance in the zonal scheme based on Rudistid lamellibranchs in the Cretaceous of western Europe by Douvillé and Toucas (Haug, 1920, p. 1171). In other circumstances such zoning is difficult.

In richly fossiliferous formations, the technique of collecting for zonal division produces large collections of different species. Sometimes a zonal scheme emerges based on assemblages of a favoured group of organisms and short-term policy may determine that the remaining fossils receive scant attention. Some of the earlier palaeontologists set us a good example by describing and illustrating whole faunas and it seems desirable that we should emulate their example as occasion arises. In this manner data will accrue useful for faunal assemblage analysis. By comparison with earlier and later faunas in the local area and further comparison with successions in other areas, the locally based populations will be readily discriminated from the migrants and the probable sources of the latter may be deduced.

Having indicated some of the influences of palaeontology on stratal classification, I conclude by recalling that whilst the geological map is the published outcome of a particular local geological study, in Britain it is regrettably rare to find in the accompanying account pictures of the fossils on which critical age determinations are based. Lists of names, helpful though they may be, lack the factual reality of even a single plate illustrating fossils! On the other hand, it should be remembered that the usual large-scale geological map is a research document showing the distribution of particular lithological units which is required to serve many purposes, some non-geological. The units shown on that map typically bear local names and require palaeontological data to ensure their correct correlation and a correct interpretation of their disposition; but the mapped divisions should not be too subjectively palaeontological. Palaeontology takes a large part when that map is combined with others for reduction to a smaller scale and when the local lithological units become subordinated to larger divisions based more particularly on their fossil content.

In questions of regional correlation the application of palaeontological knowledge of the faunas and floras in different organic provinces has had and will have important bearing on geological research.

References

ARKELL, W. J. (1933) : *The Jurassic System in Great Britain*. Oxford.

— (1946) : 'Standard of the European Jurassic.' *Bull. Geol. Soc. Amer.* **57**, 1–34.

BANCROFT, B. B. (1933) : *Correlation-Tables of the Stages Costonian-Onnian in England and Wales*. Blakeney, Glos.

— (1945) : 'The Brachiopod Zonal Indices of the Stages Costonian to Onnian.' *J. Paleont.* **19**, 181.

BELT, T. (1867) : On the 'Lingula Flags' or 'Ffestiniog Group' of the Dolgelley District. *Geol. Mag.* **4**, 493–5, 536–43.

BERRY, E. W. (1941) : *Paleobotany in Geology 1838–1938, Fiftieth Anniversary Volume of Geological Society of America*.

BUCKMAN, S. S. (1898) : 'The Grouping of some Divisions of so-called "Jurassic" Time.' *Quart. J. Geol. Soc.* **54**, 443.

— (1902) : 'The Term "Hemera."' *Geol. Mag.* [4], **9**, 563–6.

COBBOLD, E. S. (1927) : 'The Stratigraphy and Geological Structure of the Cambrian Area of Comley, Shropshire.' *Quart. J. Geol. Soc.* **83**, 551.

CUVIER, G. & BRONGNIART, A. (1811) : *Essai sur la Géographie Minéralogique des environs de Paris*. 2nd Ed. [Re-issue of 1808 pub.]

DESHAYES, P. (1831) : *Description de Coquilles caractéristiques des Terrains*. Paris.

FÜCHSEL, G. C. (1762) : 'Historia terrae et maris ex historia Thuringiae permontium descriptione erecta.' *Acta Acad. elect. Moguntiae*. Erfurt.

GEIKIE, A. (1905) : *The Founders of Geology*, 2nd Ed. London.

GIGNOUX, M. (1950) : *Géologie Stratigraphique*. Paris.

GIRAUD-SOULAVIE, J. L. (1780) : *Histoire Naturelle de la France Méridionelle*, Tome 1, 2me partie, p. 317.

GLAESSNER, M. F. (1945) : *Principles of Micropalaeontology*. Melbourne.

GORDON, W. T. (1934) : 'Plant Life and the Philosophy of Geology.' *Rep. Brit. Assoc.* (Aberdeen), 49–82.

HAUG, E. (1920) : *Traité de Géologie*. Paris.

HÉBERT, E. (1872–5) : 'Ondulations de la Craie dans le Bassin de Paris.' *Bull. Soc. Géol. France* [2], **29**, 446–72, 583–94; [3], **3**, 512–46.

HEDBERG, H. D. (1954) : 'Procedure and Terminology in Stratigraphic Classification.' *C.R. XIXme Congr. Geol. Internat.* [In press.]

JONES, O. T. (1928) : '*Plectambonites* and some Allied Genera.' *Mem. Geol. Surv. Palaeont.* **1**, pt. 5, 367–527.

JUKES-BROWNE, A. J. (1912) : *The Student's Handbook of Stratigraphical Geology*. London.

KNOPF, A. (1949) : 'Time in Earth History' in G. L. Jepson, E. Mayr and G. G. Simpson. *Genetics, Paleontology and Evolution.* Princeton, N.J.
LAMARCK, J. B. (1802) : *Hydrogéologie ou Recherches...* p. 56. Paris.
LAPWORTH, C. (1878) : 'The Moffat Series.' *Quart. J. Geol. Soc.* **34**, 240–346.
— (1879) : 'On the Tripartite Classification of the Lower Palaeozoic.' *Geol. Mag.* [2], **6**, 1–15.
LEHMANN, J. G. (1756) : *Versuch einer Geschichte von Flötz-Gebürgen.* Berlin.
LHUYD, E. (1699) : *Lithophylacii Britannici Ichnographia.* London.
LYELL, C. (1833) : *Principles of Geology*, **3**, 1st Ed. London.
— (1867) : *Principles of Geology*, 10th Ed. London, pp. 300–01.
MARR, J. E. (1898) : *The Principles of Stratigraphical Geology.* Cambridge.
MATTHEW, W. D. (1914) : 'Time Ratios in the Evolution of Mammalian Phyla. A Contribution to the Problem of the Age of the Earth.' *Science*, N.S. **40**, 232–5.
OPPEL, A. (1856–8) : *Die Juraformation Englands, Frankreichs und des südwestlichen Deutschlands.* Stuttgart.
ORBIGNY, A. d' (1842–50) : *Paléontologie Française Terrains Jurassiques, I, Cephalopodes.* Paris.
— (1851) : *Cours élementaire de Paléontologie et de Géologie Stratigraphique*, **2**, 251. Paris.
PHILLIPS, J. (1841) : *Figures and Description of the Palaeozoic Fossils of Cornwall, Devon. . . .* London.
— (1844) : *Memoirs of William Smith.* London.
ROWE, A. W. (1899) : 'An Analysis of the Genus *Micraster*.' *Quart. J. Geol. Soc.* **55**, 494.
SCHINDEWOLF, O. H. (1928) : 'Die Liegendgrenze des Karbons im Lichte biostratigraphischer Kritik.' *C.R. Congr. Strat. Carbonifère Heerlen, 1927.*
— (1950) : *Grundlagen und Methoden der paläontologischen Chronologie.* Berlin.
— (1950A) : *Grundfragen der Paläontologie.* Stuttgart.
SEDGWICK, A. & MURCHISON, R. I. (1840) : 'On the physical structure of Devonshire. . . .' *Trans. Geol. Soc.* [2], **5**, 633–705.
SOLLAS, W. J. (1905) : 'The Influence of Oxford on the History of Geology' in *The Age of the Earth.* Oxford.
SPATH, L. F. (1931) : 'On the Contemporaneity of certain Ammonite Beds in England and France.' *Geol. Mag.* **68**, 183.
WHITTINGTON, H. B. & EVITT, W. R. (1954) : 'Silicified American Trilobites.' *Spec. Mem. Geol. Soc. Amer.* **59**.
WILLIAMS-MITCHELL, E. (1948) : 'The zonal value of Foraminifera in the Chalk of England.' *Proc. Geol. Assoc.*, **59**, 91–112.
ZITTEL, K. A. von (1901) : *History of Geology and Palaeontology* (transl. M. M. Ogilvie-Gordon). London.

ARCHAEOPTERYX AND EVOLUTION

Address by

SIR GAVIN R. DE BEER, F.R.S.

PRESIDENT OF SECTION D

No man who has the honour to preside over the Zoological Section of the British Association meeting in Oxford can be unmindful of two facts: the first is the memorable meeting of 1860, when T. H. Huxley championed Darwin's recently propounded theory of evolution by means of natural selection, and blazed for it a trail which has now become one of the greatest ornaments of intellectual endeavour. The other is the fact that this meeting is being held in the Department which was presided over for many fruitful years by that great master of zoology, Edwin Stephen Goodrich. For half a century he devoted himself to the study of comparative anatomy and embryology with the object of providing evidence of the course which evolution has taken. Those of us who had the privilege to know him have reason to be proud.

In searching for a subject on which to address you to-day, it is therefore not unfitting that I should select one bearing on the subject of evolution, though without forgetting that in four years time we shall be celebrating the centenary of the first publication of the paper by Darwin and Wallace which was one of the most epoch-making events in the history of science and humanity.

In view of the great day shortly to come, it will not be expected of me that I should devote myself to a treatment of the whole subject of evolution even if it were possible in the time at my disposal, or with the knowledge at my command. I have in consequence selected a more restricted field, and one in which I have been engaged in my own researches.

I have a certain apprehension in speaking on any scientific subject, particularly that of evolution, without having some solid object to talk about so as to anchor my words to a firm sea-bed of evidence, and the material which I have selected for this purpose is the specimen of the fossil *Archaeopteryx lithographica* preserved in the British Museum (Natural History); probably the most precious, the most beautiful, and the most interesting fossil hitherto discovered in the world.

It is over 90 years since it was described by my predecessor Richard Owen, and since then a few contributions have been made to our knowledge of it; but when I came to examine it, and applied to it modern methods of research such as photography under ultra-violet illumination, I considered that it was worthy of a complete re-investigation. It is some of the results of this work which I am putting forward before you to-day for the first time, with the object not only of describing the new facts which have come to light, but also of providing evidence on which I hope to demonstrate a principle of evolution which deserves further attention.

ARCHAEOPTERYX AND THE TRANSITION FROM REPTILES TO BIRDS

The fossil is preserved in a slab and corresponding counter-slab of a block of limestone from the Solnhofen deposit of the Jurassic, about 150 million years old. At the very first glance its most important features become apparent, for while some of them are thoroughly characteristic of reptiles, others are no less completely characteristic of birds. It is this intermediate position which *Archaeopteryx* occupies that makes it an object of such enormous interest.

First the reptilian features:

(i) the long tail of 20 vertebrae, all of them free up to the tip as would be found in a reptile;

(ii) the simple articulation between the vertebrae without any of the complications found in birds ;
(iii) the short sacrum, involving no more than six vertebrae by means of which the vertebral column is attached to the pelvic girdle ;
(iv) the free metacarpal bones in the hand, and the presence of claws on all three fingers ;
(v) the free metatarsal bones in the foot ;
(vi) the simple ribs and gastralia in the ventral body wall ;
(vii) the simple brain with elongated, slender cerebral hemispheres, optic lobes, and a small cerebellum.

All these are characters which would not be in the least out of place if found in any reptile. On the other hand, there are a number of features in *Archaeopteryx* which are absolutely characteristic of birds :

(i) First the feathers, the impressions of which show that they were composed of rachis and barbs forming the vanes, identical in structure with those in modern birds.
(ii) Next comes the fact that these feathers are arranged on the forearm to form a wing, in a manner again precisely similar to that which is found in modern birds. There is a group of flight-feathers called the primaries, inserted on the hand and wrist, and another group called the secondaries, inserted on the forearm ; covering the bases of these flight-feathers are coverts.
(iii) Then there is the fact that the two collar-bones are joined to form a merry-thought, a character found only in birds.
(iv) In the pelvic girdle the pubes are directed backwards, again as in other birds.
(v) Lastly, the foot shows that the big toe was opposed to the other three toes, which is the characteristic adaptation by means of which birds perch on twigs of trees.

From the evidence already presented it is clear that *Archaeopteryx* was an arboreal animal, and that it had the power of gliding through the air supported by the flight-feathers on its wings and those of its long tail.

For many years a search has been made for the sternum. The determination of this structure is of the greatest importance, since the absence or presence on the sternum of a keel is the evidence on which to conclude that the bird had weak or strong pectoral muscles, and either was not or was able to carry out vigorous and active flight. The sternum has now been revealed and shows no sign of any keel, so it must be concluded that *Archaeopteryx* glided rather than flew.

It may be of interest to show how the sternum was discovered. For this purpose it is necessary to look at a photograph of the counter-slab taken with ultra-violet illumination under which conditions bones are fluorescent while the matrix is not. Where with ordinary light there was little to be seen, under ultra-violet light it was at once clear that a hitherto unrecognised element was there in the form of a thin bony shell torn away from the main body of the bone on the main slab. The corresponding place was then examined on the main slab. Between the left humerus and the left scapula a structure immediately became apparent and attracted attention. Having once been found, the structure was further revealed on the main slab by scratching away the matrix with fine chisels and needles. The sternum, for that is the only thing that it could be, was poorly ossified and must have been largely cartilaginous during life. Indeed, its spongy structure implies this. There remained to be supplied the proof that the structure in question is in fact bony, and this can fortunately be done by means of X-rays. There is a sufficient difference in density between the phosphate in the bones and the carbonates of the limestone matrix for it to be possible to reveal it by X-rays with suitable dosage, and the sternum throws its shadow just like the other bones of the skeleton.

Turning now to the brain ; when photographed under ordinary light on the slab, it shows the natural cast as seen from the right side. The right cerebral hemisphere and the right optic lobe are beautifully revealed because the right frontal and

parietal bones are detached and lie on the counter-slab. When photographed under ultra-violet light the brain-cast is seen to be surrounded by a number of bones, among which can be identified the left frontal and parietal, the auditory capsule, and the occipital region. On the counter-slab the right frontal and parietal are beautifully preserved, and the hollow which they form fits perfectly over the shape of the right cerebral hemisphere and optic lobe on the main slab. Ultra-violet light confirms the bony nature of these structures.

The most interesting feature in the brain is the small size of the cerebellum which did not as in modern birds encroach on the space occupied by the optic lobes. In this respect the recent investigations confirm the observations of T. Edinger (1926). There is an important correlation between the absence of a keel on the sternum, poor power of flight, and the small size of the cerebellum. It is the acrobatic power of flight of modern birds, made possible by the insertion of the powerful pectoral muscles on the keel of the sternum, that necessitates the development of a large cerebellum to co-ordinate the motor activities. None of this had yet happened at the level of evolution represented by *Archaeopteryx*.

Other anatomical details may be illustrated briefly. Among the new structures discovered on the counter-slab is a vertebra which for the first time reveals the concave and simple articular surface of the centrum.

Sir John Evans discovered in 1862 that *Archaeopteryx* possessed teeth on the premaxilla and maxilla. Ultra-violet light shows the details of these structures, and the way in which the teeth are fitted into their sockets and have a little pediment, like the base of a column. The presence of teeth in *Archaeopteryx* is, of course, not unique among birds since the Cretaceous fossils *Hesperornis* and *Ichthyornis* possessed them.

Transition : Conversion or Mosaic?

From what has already been said, it is clear that *Archaeopteryx* provides a magnificent example of an animal intermediate between two classes, the reptiles and the birds, with each of which it shares a number of well-marked characters. It is, however, worth while spending a little more time in considering what precisely is meant by the statement that an animal is intermediate between two classes.

If we look at almost any part of a living vertebrate animal, we can tell at once which class the animal belongs to : skin, skeleton, brain, heart, kidneys, are all stamped with the identity of their class. This shows that in the evolution of the classes of vertebrates as we know them alive to-day, all the parts of their bodies have undergone modification. This might suggest that in the evolutionary process which converted animals of one class into the next, there was a gradual and general transformation of the whole animal. If the evolution of the vertebrate classes involved processes of this nature, and an animal was caught in the transitional stage, the parts of such an animal would be intermediate in structure between the two classes.

On the other hand, the statement that an animal was intermediate might mean that it was a mixture and that the transition affected some parts of the animal and not others, with the result that some parts were similar to those of one type, other parts similar to the other type, and few or no parts intermediate in structure. In such a case the animal might be regarded as a mosaic in which the pieces could be replaced independently one by one, so that the transitional stages were a jumble of characters some of them similar to those of the class from which the animal evolved, others similar to those of the class into which the animal was evolving.

If now it be asked which kind of transition is shown by *Archaeopteryx*, the answer is perfectly clear. It is a mosaic in which some characters are perfectly reptilian, and others no less perfectly avian. In its evolution from its reptilian ancestors, therefore, the modifications which it has undergone have affected some structures to produce their complete transformation while other structures have not yet been affected at all. In the subsequent evolution from *Archaeopteryx* to the conditions found in modern birds, the latter structures, in their turn, have been affected with the result that in the brain, vertebrae, sacrum, and carinated sternum, modern birds

have got even further away from the condition of their original reptilian ancestors than was *Archaeopteryx*.

The condition in *Archaeopteryx* may most conveniently be shown in tabular form :

ARCHAEOPTERYX

Reptilian characters :	*Avian characters :*
simple brain with small cerebellum	feathers
long tail of separate vertebrae	arrangement of feathers
simple articulation of vertebrae	fused clavicles
short sacrum	pubes directed backwards
free metacarpals	opposable hallux
free metatarsal	
simple ribs	
gastralia	

If the transition from reptiles to birds was characterised by what I propose to call *mosaic evolution*, it becomes of interest to enquire whether the same mode was followed in the transition between other classes of vertebrates, and to make a general investigation into the evolution of the classes of jawed vertebrates alive to-day.

Vertebrate animals present themselves to the eye and to the mind as disposed along five great levels, or shelves extending one above the other along the walls of time. The lowest shelf is for the fishes, the second for amphibia, the third for reptiles, and the fourth and fifth for birds and mammals. In each case, except for the last, the definition of the contents of the shelf is based on the fundamental structural and functional requirements of adaptation in a broad way to the medium or media in which the animals live. A life wholly spent in water is the governing factor for the fish ; early life in water followed by life on land for the amphibia (although some may secondarily return to an aquatic existence) ; complete emancipation from early larval life in water for the reptiles (although some may become secondarily adapted to an aquatic medium) ; ability to fly in the air by means of feathers for the birds. The mammals are exposed to the same ecological problems as the reptiles, and if they are placed on a shelf above them, it is because their basic adaptation to their problems is on a level so much more efficient that an arbitrary but accepted convention has sanctioned this practice and used mammary glands and hair as diagnostic features in living forms, although it may well be that these structures were also present in the ancestors of the mammals while they were still undoubted reptiles.

In each case, only one passage from a shelf to the next has been successful and given rise to forms which have survived as the classes of vertebrates alive to-day. As Simpson (1951) has stressed, it is because they have successfully survived and radiated in their newly conquered environments that these groups have become classes. If they had failed to " break-through " onto a higher shelf, like the numerous other unsuccessful competitors, they would not have given rise to a class. If the pterodactyls had established themselves in the mastery of the air, they would no doubt be regarded as a class, but they went extinct.

So firmly are the differences between the various grades of vertebrate animals impressed on the student, that there is a tendency to imagine that the ' hard work ' of evolution consists in the transition from one shelf to that above it. There is here the possibility of error, for if evolution is measured by the quantity of change observed between the starting and ending points of a given line of descent, ' more ' evolution may take place on and within each shelf than in the passage from one shelf to the next. Within each shelf, evolution involves adaptive radiation which diversifies the animals to a very great extent as the different lines become more and more closely adjusted to special environments. The reptiles show this well with their astonishing diversity of form and size. There are also the trends

of evolution which Watson (1951) has shown to occur in each level, unrelated to any specific adaptation of the environment, but progressing in a secular manner with time. Quite apart from the transitions from one shelf to the next, there is a very wide tolerance for the amount of change of size, shape, proportions, accretion or loss, which a strain of evolving animals may show.

In the case of the transitional passages from one shelf to the next, the starting and ending points of the evolutionary progress are known and definable. From one known condition the animals must have arrived at another, not very different from it but possessing a few fundamentally important characters which define the level of the next shelf.

If the method of transition from reptiles to birds, as shown in *Archaeopteryx* be of general applicability, it should be possible by direct appeal to the facts of observation to demonstrate its occurrence in the case of other great transitions, from fish to amphibia, from amphibia to reptiles, and from reptiles to mammals. In each case fossils are known which, although not themselves the direct ancestors of the remainder of the animals in the shelf to which the evolution is progressing, were nevertheless so closely related to them that they can be taken as examples of animals in transition. It will be of interest to consider them in a little more detail.

The Transition from Fish to Amphibian

Bony fish are alike in the possession of the following structures which not only enable them to live in their watery medium but are characteristic of them: paired fins in the form of paddles, median fins supported by cartilaginous radials and dermal fin-rays (lepidotrichia), lateral-line canals protected in tubes in the dermal bones, opercular bones protecting the branchial cavity, and a bony connexion between the hind part of the skull and the pectoral girdle. The amphibia, on the other hand, are characterised by the pentadactyl limbs; the median fins which are only found in their aquatic forms have no supporting radials or rays, and the lateral-line organs lie in grooves in the bones or simply in the skin, not in tubes in the bones; the bony operculum has vanished, as has the chain of bones connecting the skull with the pectoral girdle.

It is therefore remarkable that, as Jarvik (1952) has shown, the Ichthyostegalia present an intermediate and mixed condition in which median fins are present supported by radials and lepidotrichia, and the lateral-line organs perforate the bones in tubes. These structures are absolutely characteristic of fish. On the other hand, the Ichthyostegalia have pentadactyl limbs, absolutely characteristic of amphibia. Less sensational than these but nevertheless characteristic of one or the other of the two types between which the transition is made, are the following structures of Ichthyostegalia: presence of a pre-opercular bone, a (small) subopercular bone, and an ethmosphenoid bone, all of which are found in bony fish but not in amphibia; the presence of bicipital ribs, and the shortening of the hinder part of the skull which is unjointed and free from the pectoral girdle, all of which are characteristic of amphibia but not of fish.

If it were not for the pentadactyl limbs, the Ichthyostegalia would have to be regarded as fish instead of amphibia; and the fact that a single piece of the mosaic pattern marks the passage of the frontier between one class of vertebrates and another, suggests that such a transition can be achieved by a moderate quantity of evolutionary change.

In his Croonian Lecture, Watson (1925) has already shown that the physiological and mechanical problems that required to be solved by the ancestors of the amphibia in their passage from life in water to life on land, necessitated no new neurological or muscular machinery. The system of myotomes segmentally arranged on each side of the body which enables a fish to swim also enables an amphibian to crawl; the muscles of the floor of the mouth, which by raising it when the mouth is closed, force water back into the pharynx and out through the gill-slits in the fish, also force air down into the lungs in the amphibian; the hyomandibula which is applied to the imperforate side of the ear-capsule in fish and helps to support the upper jaw, also enables vibrations to be

transmitted to the inner ear, even without the perforation of the fenestra ovalis which the most primitive amphibia have not yet achieved. In brief, the passage from class Pisces to class Amphibia involved no upheaval or sensational reconstruction of the body. This is what would be expected if the transition took place by mosaic evolution, one piece at a time.

The Ichthyostegalia present another feature of interest and general applicability, which arises out of the fact that they cannot themselves be taken as lying on the direct line of transition from the fish to the amphibia. The reason why they cannot be so regarded is that they possess some characters which are too specialised for it to have been possible for the remainder of the amphibia to have been evolved from them. Among these characters are the structure of the occipital region, and the anterior position of the articulation between the quadrate and the lower jaw. These are characters which other amphibia show at a later period of evolution, and in which the Ichthyostegalia are precociously advanced. But the fact that an animal can at one and the same time show so many features which would make it an ideal transitional form, and also spoil this picture by possessing one or two characters which rule it out as a direct ancestor, is itself an argument in support of the principle of mosaic evolution, with the different pieces evolving separately, and some of them too fast. This phenomenon is found again and again in the study of transitions from one type of animal to another, and appears to be of general applicability. It would be more difficult to understand if the transitions took place by a gradual and simultaneous conversion of all the parts of the animal.

Conversely, there are animals which in some of their features have failed to keep pace with the general progress of the group to which they belong. A case is provided by the amphibian *Eogyrinus* which is more advanced than the Ichthyostegalia in general structure and yet retains the connection between the bones of the skull and those of the shoulder girdle which the Ichthyostegalia had already lost.

It may be of interest to have the analysis of certain characters during the transition from fish to amphibia in tabular form :—

ICHTHYOSTEGA

Fish characters :	*Amphibian characters :*
median fin	pentadactyl limbs
radials	bicipital ribs
lepidotrichia	long forepart of skull, short hind part
preopercular bone	skull free from pectoral girdle
subopercular bone (vestigial)	no joint in skull
tubes for lateral line	
ethmosphenoid ossification	*Specialised characters :*
short parasphenoid	structure of occipital region
	quadrate articulation too far forward

THE TRANSITION FROM AMPHIBIAN TO REPTILE

In the transition from amphibian to reptile the essential difference between them is that whereas the amphibia[1] never emancipated themselves completely from water, which is necessary for the sperm to find the egg (in the absence of internal fertilisation), for the egg and larva to develop in, and for the skin to breathe, the reptiles succeeded in getting completely free from water. This they did by the evolution of copulatory organs allowing of internal fertilisation, the evolution of embryonic membranes (chorion, amnion, and allantois) which made it possible for the embryo to develop within a fluid medium (in the amniotic cavity) as a closed system (the cleidoic egg) inside a shell on dry land or in the oviduct ; and by the acquisition of an efficient mechanism of pulmonary

[1] An exception must be made of the Caecilians which although still amphibians have acquired independence from water by means of internal fertilisation and many of them are viviparous. They are, however, so highly specialised that they cannot be said to have made a new ' shelf.'

respiration with the ribs and intercostal muscles expanding the lungs.

The reptilian emancipation from water is reflected anatomically in the fact that the reptilian skull shows no grooves for lateral-line canals. The other structural characters diagnostic of reptiles are hard to define and involve minute details. Some of them are given in the table below. One of them is remarkable : the lower jaw hinges by means of the articular bone, an independent ossification, characteristic of no amphibian and all reptiles. *Seymouria* which is an intermediate form long regarded as the most primitive known reptile, possesses the articular bone, but it has now been shown to have lateral-line canal grooves, and was an amphibian. It has just not got on to the new shelf. Nevertheless, D. M. S. Watson's (1919, p. 291) remarks apply with no less force than when he wrote them (before it was recognised that *Seymouria* was an amphibian): ' In every part of its skeleton,' he wrote, ' it shows a mixture of Temnospondyl [amphibian] and Reptilian characters, each recognisable, in general showing little evidence of an intermediate condition. The whole effect of its structure is that of a mosaic of separate details, some completely amphibian, some completely reptilian, and very few, if any, showing a passage leading from one to the other.'

It is clear, therefore, that the evidence from the study of the transition from amphibia to reptilia is in favour of the mosaic mode of evolution.

Seymouria also illustrates a further consequence of this type of evolution. Just as in some cases (e.g. *Ichthyostega*) an animal may show characters which have evolved too fast relatively to the other characters, in other cases certain characters may have been left in a profoundly archaic condition. An example is provided in *Seymouria* by the presence of a procoracoid without a coracoid, and a large tooth on the third coronoid. These characters are those of Osteolepid fish.

SEYMOURIA

Amphibian characters :

reticulate ornamentation of skull
lateral-line canal grooves present
no supraoccipital bone
large opening from braincase to inner ear
massive paroccipital process directed upwards
tabular flange covering paroccipital
palatine with tooth in pit
ectopterygoid with very weak flange facing lower jaw
shape of large pterygoid
maxillary teeth with alternate replacement, with fluted roots, fused to their base and to a labial wall of bone
deep otic notch
intertemporal bone present
quadrate inclined backwards
mandible with postsplenial and 3 coronoids
axis vertebra resembling one behind it
intercentra with process for head of rib
single sacral vertebra
separate intermedium in tarsus

Pre-Amphibian characters :

large tooth on 3rd coronoid
procoracoid but no coracoid.

Reptilian characters :

foramen ovale large, low down, bordered by basipterygoid process
lachrymal extends from orbit to nostril
choanae near midline
articular separate from supra-angular
basioccipital largely exposed ventrally
large tubera basisphenoidales
lower jaw hinges by articular, an independent ossification
odontoid process a single ossification
neural arches wide and swollen
articular surface of zygapophyses horizontal
digital formula 2–3–4–5–4
humerus with entepicondylar foramen.

The Transition from Reptile to Mammal

The transition from the reptilian to the mammalian shelf of evolution is in many ways the most difficult to study. This is partly because fossils which might be regarded as ancestral to the existing mammals have not yet been found ; partly because the mesozoic mammalian fossils that are known belong to unrelated groups which left no descendants ; and partly because many of the features commonly accepted as characteristic of mammals were present in the group of theromorph reptiles that evolved parallel with the mammals. For example, the advanced therapsid reptiles show the following features : in the skull two occipital condyles, a bony false palate, disappearance of the pineal foramen and of the postorbital bar, appearance of turbinal bones, and a large dentary with an ascending ramus; teeth differentiated into incisors, canines, and molars, with single replacement ; in the skeleton of the limbs and girdles, the loss of the cleithrum, the forward rotation of the ilium, the presence of the olecranon process on the ulna, the tuber calcis in the foot, and the reduction of the digital formula to 2–3–3–3–3 ; in the brain a cerebellum with vermis, flocculi, and pons varolii.

Thanks to the work of R. Broom (1932), C.-C. Young (1947) and W. G. Kuehne, it is known that in the Triassic therapsid group of Ictidosauria, *Tritylodon* from South Africa, *Bienotherium* from China, and *Oligokyphus* from England, show still closer resemblance to mammals in that their skull has lost the prefrontal, postfrontal, and postorbital bones, the molar teeth have two roots, and the acromion process on the scapula is present. The only feature left which shows that the Therapsida including the Ictidosauria were still reptiles, according to the current definition, is the articulation of the lower jaw by means of the articular and quadrate bones, with the correlated condition that the columella auris was the only auditory ossicle and the angular remained in the lower jaw and had not been transformed into a tympanic bone.

The designation of the above-mentioned characters as mammalian is therefore true only when consideration is restricted to existing and living reptiles and mammals. They are not diagnostic characters and should be called mammal-like. When all the fossil forms are considered, as they must be, the only diagnostic feature of the mammals as currently accepted is the articulation of the lower jaw by means of the dentary and squamosal, and the conversion of the quadrate and articular into the incus and malleus and of the columella auris into the stapes.

Living mammals are warm-blooded and have hair which enables them to minimise the loss of heat ; it is almost certain that this was also true of the higher theromorph reptiles, because of the agility which their structure argues and the presence of turbinals whose function it is to warm the air in its passage through the nose to the lungs. It is probable that these reptiles also had two ventricles, non-nucleated red blood-corpuscles, and a diaphragm, all of which are features which raise the efficiency of the circulatory and respiratory system. What cannot be inferred from the fossilised parts, however, is whether they were viviparous, and had mammary glands, and a single left aortic arch. Judging from the monotremes, the ancestors of the mammals lacked the first of these characters but possessed the last two.

In the evolution of the other classes of vertebrates there has in each case been a clean-cut and sudden adaptation to a new medium : the partial and total conquest of dry land, and the mastery of the air. Each of these shelves of vertebrate evolution can be characterised, therefore, not only by the diagnostic structural features, but also by a habitat. In the case of the evolution of the mammals this was not so. In the production of the mammals there was no conquest of a new medium : primitive reptiles and mammals alike inhabited the land. Mammals are only perfected reptiles, more efficient, better adapted, and possessed of a greater degree of independence of their environment.

It follows that mammals were able to evolve slowly and gradually by progressive modifications here and there all over the body ; and the point at which the distinction between reptiles and mammals is placed is purely arbitrary. In any case the diagnostic feature commonly accepted,

the squamoso-dentary articulation of the lower jaw, cannot be correlated with the the solution of any major problem of adaptation as was the case with the pentadactyl limb, the absence of lateral-line canal grooves, and the feathers. What this change did was to enable the mammals to chew and to hear more efficiently.

If the possession of hair were taken as the mammalian criterion, indicating a homoeothermous condition, and if it could be demonstrated in fossils, most of the higher therapsid reptiles would almost certainly be included in the mammals. The selection as a criterion of a feature which evolved late (the squamoso- dentary articulation), has had the result of relegating the important phases of mammalian evolution to the pre-mammalian stage. It follows that the reptiles amongst which the ancestors of mammals must be sought were already almost full of mammal-like characters. These reptiles were the Ictidosauria, and it is largely because they were Triassic and therefore too late that they cannot be regarded as directly ancestral to the mammals. The Ictidosauria must, however, have resembled these ancestral forms, and when their characters are tabulated it is seen that a few of them are fairly ancestral or reptilian while most are equally definitely progressive or mammal-like :

Instead therefore of presenting a picture in which a small number of pieces conform to the new type showing a striking mosaic, most of the pieces belong to the new type and only a few reflect the old : the mosaic has almost been converted into a self-coloured pattern.

There is another possibility of studying the transition from reptiles to mammals because there are mammals alive to-day which, although so highly specialised that they cannot be regarded as ancestral to any other mammals, also show a number of features which they must have inherited from the earliest mammals. These are the monotremes, and it is remarkable that they also show a mosaic of reptilian and mammalian characters, all the more interesting because they include soft parts.

Thoroughly reptilian are the oviparous type of reproduction with yolk in the egg and a shell ; the presence of the egg-tooth and caruncle used for hatching ; the presence of coracoids, procoracoids and interclavicle ; and the absence of a corpus callosum from the brain.

On the other hand, the presence of hair, mammary glands, non-nucleated red blood-corpuscles, the diaphragm, single left aortic arch, squamoso-dentary articulation of the lower jaw, three auditory ossicles, and seven cervical vertebrae, are equally definite mammalian and mammal-like characters.

ICTIDOSAURIA

Ancestral reptilian characters :	*Progressive mammal-like characters :*
quadrate-articular articulation	two occipital condyles
columella auris simple	bony false palate
post-dentary bones in lower jaw	no pineal foramen
interclavicle present	no postorbital bar
coracoid present	no prefrontal bone
procoracoid present	no postorbital bone
	dentary large with ascending ramus
	turbinal bones
	teeth heterodont, diphyodont, molars with 2 roots
	no cleithrum
	olecranon process present
	acromion process present
	tuber calcis present
	ilium rotated forward
	digital formula 2–3–3–3–3
	cerebellum with vermis, flocculi, and pons varolii.

In other words, the monotremes have been produced from their reptilian ancestors by mosaic evolution and have remained in a condition in which some of the oldest reptilian pieces of the mosaic are preserved:

pected to have acted with greater rigour against organisms varying in more than one direction at a time, unless the directions were correlated, as was the case with the evolution of limbs and teeth in some horses, for example.

ORNITHORHYNCHUS

Reptilian characters:	*Mammalian characters:*
Oviparous reproduction with shell and yolk	hair
egg-tooth and caruncle	mammary glands
squamosal canal	non-nucleated red blood-cells
coracoid, procoracoid, interclavicle present	diaphragm
no corpus callosum	single left aorta
no pinna to ear	squamoso-dentary articulation
no cribriform plate	3 auditory ossicles
taenia clino-orbitalis present	7 cervical vertebrae

CONCLUSION

It has now been seen that in the transitions between classes of living vertebrates, there is evidence that morphological evolution has progressed by means of the mode here described as mosaic. The alternative type of change, involving gradual and general conversion of the whole organism must have occurred in regard to such characters as the chemical constitution of the body-fluids, and may have done so in the case of the secular trends described by Watson. It does not seem to have occurred in the evolutionary changes which lifted animals from one class of vertebrates to the next.

It is possible that this is connected in some way with what, following Waddington, we can call the epigenetic climate of a developing organism. Owing to the genetic make-up and the internal conditions of development in an organism, there may be only a limited number of changes possible, and it is legitimate to think that changes restricted to single organ-systems are more likely to have occurred than progressive changes affecting the entire organism. It is further probable that changes restricted to single organ-systems at a time were more viable and less likely to succomb to selection. Organisms are delicately balanced and adjusted mechanisms, and on the average, changes are more likely to upset than to strengthen them. Selection may therefore be ex-

It has long been held by palaeontologists that the different parts of organisms are capable of independent evolution, proceeding at different rates. This emerges very clearly from studies of rates of evolution as presented by G. G. Simpson (1953), and may appear to be so well known as to make this paper redundant. Perhaps it is, but it may be worth stressing that the principle of the independence of characters, shown, for example, in tooth-length and tooth-height in the evolution of the horses, also applies to the evolution of the classes of the vertebrates.

A necessary consequence of mosaic evolution and of the independence of characters evolving at different rates is the production of animals showing mixtures of primitive and specialised characters.

It is a commonplace of palaeontology that in the search for ancestral forms of groups, the fossils which very nearly fill the bill usually show one or more characters that rule them out from the direct ancestry of the descendants, either because they have already lost some structure which the early members of the descendant group still have, or because they possess some structure which the early members of the group in question have not yet evolved. An example of the latter is *Ichthyostega* where the position of the articulation of the lower jaw is more advanced than in the earliest Amphibia.

In each case the fossil gives a very good

approximation of the conditions that must have obtained in the real ancestor, but the picture has been spoiled by the precocious evolution of a single character or two, in which the animal anticipated the condition found in the later stages of the evolution of the group in question. A useful term to denote characters of this sort has been coined by K. P. Oakley. Following him, a character in which an animal anticipates a later stage of the evolution of a group (with which it must therefore be evolving in parallel) may be called ecgonomorphic (resembling the descendant).

Conversely, there are cases where characters have survived little-modified from the ancestral state through later stages of evolution. An example is provided by the tooth on the 3rd coronoid of the lower jaw in *Seymouria*, in which it resembles no other known amphibian but reflects the conditions in Osteolepid fish. Following E. I. White such a character may be called progenomorphic (resembling the ancestor). The Ictidosauria as a group may be regarded as progenomorphs, for no structural features of theirs would rule them out from the ancestry of mammals if they occurred earlier in the fossil record. It must be concluded that they have preserved those ancestral features almost without change. The same may be true of *Seymouria* which was too late to be ancestral to the reptiles.

Only in the case of *Archaeopteryx* is it permissible to say that no time-relations or structural features have yet been found to disqualify it from being regarded as ancestral to a class, viz. birds.

Finally there is the fact that one and the same animal may show primitive and specialised characters in different parts of its body. This was recognised by W. D. Matthew, who applied the term ' compensation ' to this condition which is, of course, only another case of mosaic evolution, in which the organism shows both ecgonomorphic and progenomorphic characters. An example given by Watson (1951) is that of the Permian Labyrinthodont *Trimerorhachis* in which the condition of the occipital condyle is extremely primitive (like the Carboniferous forms), but the flattening of the skull very specialised (like the Triassic forms).

The significance and wide application of mosaic evolution as a general principle was clearly recognised by D. M. S. Watson thirty-five years ago when he wrote (1919, p. 300) : ' The curious way in which the structure of *Seymouria* is built up of perfectly well developed amphibian characters and equally decisive reptilian features, those of intermediate type being very rare, affords a magnificent example of the way in which the evolution of great groups may have taken place.' I believe that it has, and therefore propose to give to the mosaic mode of evolution the name of Watson's Rule.[1]

Bibliography

Broom, R. (1932). *The Mammal-like Reptiles of South Africa and the Origin of Mammals.* London, 1932.

Edinger, T. (1926). ' The Brain of Archaeopteryx.' *Ann. Mag. Nat. Hist.*, Ser. 9, vol. xviii, 1926, 151.

Jarvik, E. (1952). On the fish-like tail in the Ichthyostegid Stegocephalians with descriptions of a new Stegocephalian and a new Crossopterygian from the Upper Devonian of East Greenland. *Meddel. Grönland*, Bd. 114, No. 12, Copenhagen, 1952.

Oakley, K. P. (1952). ' Swanscombe Man.' *Proc. Geol. Soc.*, London, **63**, 1952, 271.

Simpson, G. G. (1945). ' The Principles of Classification and a Classification of Mammals.' *Bull. Amer. Mus. Nat. Hist.*, **85**, New York, 1945.

—— (1953). *The Major Features of Evolution.* New York, 1953.

Watson, D. M. S. (1919). On Seymouria, the most primitive known reptile. *Proc. Zool. Soc. Lond.*, 1919, 267.

—— (1925). ' The Evolution and Origin of the Amphibia.' *Philos. Trans. Roy. Soc.*, Ser. B, **214**, 1925, 189.

—— (1951). *Palaeontology and Modern Biology.* New Haven, 1951.

White, E. (1952). ' Australian Arthrodires.' *Bull. Brit. Mus. (Nat. Hist.)*, **1**, 1952, 251.

Young, C.-C. (1947). ' Mammal-like Reptiles from Lufeng, Yunan, China.' *Proc. Zool. Soc.* Lond. **117**, 1947, 537.

[1] It is a pleasure to record the help and criticisms I have received in this work from Prof. A. C. Hardy, Dr. H. W. Parker, Dr. E. I. White and Dr. W. E. Swinton.

THE COAST AND THE GEOGRAPHER

Address by

PROFESSOR J. A. STEERS

PRESIDENT OF SECTION E

ALTHOUGH the great floods on the East Coast took place just over a year and a half ago, their effects are still much in our minds. The nation for a short time became extremely conscious of its coast, and geographers and engineers in particular found much to do and to study. It is on the geographical interests and problems of coastal matters that I propose to address you to-day.

My introduction to these problems was in March, 1923, when, following a suggestion made to me by Philip Lake, I spent some weeks at Orford, in Suffolk, in order to study the nature and formation of Orford Ness. A little previous to that date Douglas Johnson, in America, had published his classic work on *Shore Processes and Shoreline Development*. This represented a considerable advance in our knowledge of coastal matters, and brought together the scattered literature on the subject. The book was intended as the first of a trilogy, and some years later was followed by a detailed study of *The new England-Acadian Shoreline*. The final volume was never written. Johnson's work was an inspiration to all interested in coastal physiography.

In the study of Orford Ness it soon became obvious that history and ecology should be given equal weight with purely physical investigations. Careful investigation of the numerous ridges which make up this formation gave the clue to the way in which the spit was built, but offered little indication of time, because it was clear from quite recent changes at its southern end that any simple average figure of longitudinal growth led to a very erroneous conclusion It so happened that old maps and charts, descriptions of the coast, and a particularly fine estate map by Norden, enabled me to connect with reasonable certainty historical evidence and physical form. This interrelation is now taken for granted, and was, of course, well known to many earlier investigators ; in taking the illustration of Orford Ness I do so only to emphasise the point and also to direct attention to the desirability of more work of this kind. At Harlech, at Winterton Ness in Norfolk, and at several other places, the historical evidence so far available is far less both in quantity and quality, than that for Orford. In places like Dungeness, the district around Sandwich in Kent, the Culbin Sands on the Moray Firth, and some others, direct historical evidence of the evolution of coastal forms is considerable. One of the lines of investigation open to a research worker in historical geography might well be to analyse the Courts of Sewers, Manorial records, both lay and ecclesiastical, and other such documents and to collect together any references to the coast. That this may be a somewhat thankless task I am well aware, but the main existing difficulty is that the material is fragmentary and scattered, and that the student of coastal forms and processes may be unfamiliar both with the nature of some of the sources and possibly with the deciphering of papers and writings which are in mediaeval manuscript.

The ways in which physical and historical lines of enquiry so frequently dovetail, even in their simple form, make it seem extremely probable that fuller enquiries into the history of features such as Loch Strathbeg in Aberdeenshire, Morfa Harlech and Morfa Dyffryn in Cardigan Bay, or the Bridgwater Flats in Somerset, would help greatly in the elucidation of their formation.

Documentary history is, however, only a part of this problem. The border line

between history and archaeology cannot be drawn clearly, but both subjects are of vital importance to the subject under discussion. The Culbin Sands illustrate this argument in a remarkable way. These sands, now so nearly conquered and soon to be almost completely hidden by the trees planted by the Forestry Commission, attained their present general form at the end of the seventeenth century. The process of accumulation was a slow one, and no one now believes in the 'sudden' overwhelming of the area. Nevertheless, a fertile estate, including a substantial house and probably some far less substantial cottages, was finally buried about 1694. Before that time sands had been blowing for centuries to the west of the Culbin area. Remains of middens, the finding of artifacts and other pieces of evidence, show clearly that the coastal area from Nairn to Elgin was occupied from early times. Moreover, peat surfaces often found in the dunes, especially in those in Burghead Bay, just east of Culbin, testify to the somewhat spasmodic accumulation of sands.

Two of the most interesting coastal districts in which the relation between archaeology and coastal evolution can be examined are in the far north. At Skara Brae on the west coast of Mainland, Orkney, a Neolithic village has been partly destroyed by the sea but thanks to the Ministry of Works is now well protected. At the south end of Mainland, Shetland, and quite near Sumburgh Head, is the magnificent site of Yarlshof—in occupation from Neolithic to mediaeval times. Here again action against erosion has been taken by the Ministry of Works. When these stations were first occupied they were presumably near, but out of reach of, the waves. Skara Brae certainly suffered severe storms, and sand encroachment may well have led to its desertion.

The geographer, especially if he is inclined more to the physical side of the subject, will derive an infinity of interest from the north of Scotland, but unless he has also an interest in archaeological and related subjects he will miss a good deal of the significance of the finds he and others may make. The work that is now being pursued at the Sands of Forvie, on the north side of the Ythan mouth in Aberdeenshire, exemplifies extremely well the close interrelation of field work in geography, ecology, and archaeology. The establishment of Iron Age sites on the sands, the burying of part of a village, and of a chapel, all make the study of the area of deep interest and significance, but once again they emphasise the importance of joint investigations, which might well be extended to the dunes in Luce Bay, Wigtownshire, and other places.

It is not difficult to find other examples showing how history and archaeology enable us to trace back the evolution of purely physical features. The folk tale of Cantref y Gwaelod in Cardigan Bay ; the accumulation of sands in South Wales and Cornwall, and the raising of the low Roman banks along the Thames in the district of Londinium, together with the far greater development of the mediaeval banks and their modern representatives, are instances. At Brentwood occurs a superposition of Roman on Iron and Bronze Age settlements which serves as a measure of the downward movement of the land relative to the sea in pre-historical and early historical times. The reclamation of the Fens and the continuing embanking of the Wash illustrate admirably the close interconnection between historical and physical aspects of geography.

When all is said and done, however, all these factors are but the last of a long series of geological events. To try to define the limits between geography, history, archaeology, and geology is waste of time, but let us fully acknowledge that our problems often have a definite and important geological origin. The downward movement of which we have just spoken, may mean that the sea-level has risen relative to the land or vice-versa, or there may have been a combination of both land and water movement. It is well known that the lowest submerged features we find around this country suggest a recent rise of sea-level of the order of 200 feet. Of possible greater movements, such as may have been associated with the submarine canyons which occur off France and Spain, and elsewhere in the North Atlantic, and with the high-level surfaces of these and many other countries, we need not speculate to-day. But the submerged forests of Britain are inextricably

involved with the raised beaches. In the Thames estuary all the evidence suggests a sinking of the land relative to the sea in post-glacial times at any rate. The numerous raised beaches and carse lands in Scotland, and their archaeological remains, make it quite clear that the northern part of Britain has been rising relatively to sea-level. That this movement is still in action is well known. South-eastern England is sinking, north Britain is rising relatively to sea-level. The movement is small, perhaps 6–12 inches a century, but in south-eastern England it is of considerable significance since in the long run it may involve the raising and strengthening of sea-walls—a process continuous since Roman times—and a time may come when it may well have serious implications for certain docks and harbours. The absence of raised beaches in Orkney and Shetland and their rarity in Sutherland and the Outer Hebrides, indicate that in the far north as distinct from much of Scotland there may be a downward movement of the land relative to sea-level. The drowned peat and numerous freshly drowned valleys all support this view.

At this point let me acknowledge the debt geographers owe to the exponents of pollen analysis. The dating and sequence of recent deposits, especially of peat, has greatly increased our understanding of the borderline period between geology and history. Godwin's work on the Fenland of East Anglia and Somerset, and Jennings' investigations of the Broads bear witness to this in our own country. We now have a reasonably detailed and correct picture of the evolution of the coast in these former deep indentations and estuaries. It is interesting to note that many of the Broads about which geographers and others have speculated greatly in the past have now been shown by Dr. Lambert, an ecologist, to be artificial—and the physical geographer must turn to his historical colleague to throw light on the reasons for the extensive peat cuttings in mediaeval times. Another technique that will greatly help the geographer is that of dating recent deposits by Carbon methods.

The Scottish raised beaches have often been described in the Geological Survey Memoirs and elsewhere, and their nature and distribution are admirably shown on the maps of the Survey. Nevertheless much remains to be found out about them. Those of you who know Arran will remember that the raised beach platform is well developed almost all round the island. The much smaller island of Great Cumbrae is surrounded by a well-cut platform. If you sail up the Sound of Jura and visit the islands of Luing and Seil, and the sheltered waters around the small island of Torsay, and numerous other places, you will find impressive raised-beach platforms cut in hard rocks, almost horizontal, and backed by high and old cliffs. But how have these features been formed? It is extremely difficult to appreciate that the sea, fifteen to thirty feet higher than it is to-day, cut these terraces in such sheltered areas. Yet, presumably, this must be so. The western side of Arran faces the narrow Kilbrennan Sound; there is less than two miles of water between Cumbrae and the Ayrshire coast, and in the upper waters of localities like the Firth of Lorne and Jura, it is hard to understand how wave action, even in periods of rougher weather, and somewhat higher sea-level, could cut these features. Their significance from the point of view of human geography is often considerable, since many of these beaches are the sites of settlement, and often afford good cultivable land. Moreover, their very nature implies that where well-developed, they are followed by roads. Yet how little we understand them. It is curious that we have no definite information of the level at which platforms are cut. Since wave-action is undoubtedly more effective at high water, it is reasonable to argue that they are cut then and subsequently raised well above that level. A point that is relevant here is the rate at which the raised-beach platforms themselves are being eroded. I know of no measurements, nor, in fact, of any general discussion of this matter. Yet it is one of considerable importance, since the raising of the beach platform itself may be comparatively recent—at least in the case of a low beach. Behind the platform may be a high cliff, but have we any information that the sea is forming a modern platform in front of and at a lower level than the raised-beach platform? If, too,

parts of Scotland are slowly rising relative to sea-level how does that affect the problem?

A variation of this problem presents itself in many parts of England. Two examples will suffice for illustration. Along the coast of much of Kent and Sussex there is a fine platform cut in the Chalk. It appears to be mainly of present-day origin, and certainly it is covered at high water, and the cliffs in its rear are receding to-day. Along the north coast of Cornwall and Devon a magnificent shore platform is cut in contorted and hard rocks. Again, the reasonable assumption is that this is, at least in part, a product of present-day conditions, although it may well be to some considerable extent a relic of earlier conditions. But have any investigations been made as to the rate of erosion of these English platforms? Such an enquiry might well be rewarding, and if we could obtain some definite information on this matter, not only should we be in a better position to understand a little more about the very sheltered Scottish examples, but possibly light could be thrown on the wider question of what Professor Wooldridge, in his Presidential Address to this Section in 1951, called 'Upland Surfaces.'

With this investigation of raised beaches and of the platforms on which they rest, goes our enquiry into the forms and origin of cliffs. Here, too, I believe we often take too much for granted. A cliff facing the open ocean only too easily suggests cause and effect. Doubtless in most cases the cliff is a simple eroded form. But as with some other much used words we are inclined to muddle definition of the word and origin of the feature to which it applies. A cliff is a steep slope; if a cliff faces the sea it does not automatically follow that the steep slope is an erosion slope—at least in its entirety. A journey round the western coast of Scotland will show steep slopes facing water such as the Sound of Sleat, the Sound of Raasay, or the Inner Sound. Some of these are eroded forms, some stand behind raised beaches and are now protected, some may be mainly glacial in origin, some may in the first place be the result of faulting. Their present-day form must also depend amongst other things on the relative rates of marine and sub-aerial erosion, on cliff structure, on vegetation, on relative movements of land and sea-level, and many other factors.

There is another matter which is all too readily forgotten, and even if not forgotten, known in insufficient detail—the nature of the adjacent sea-floor. Physiographers often deplore the wide spacing of land contours, but the difficulties of contouring an invisible surface are considerable, and the inferences that can be drawn from such inadequate information should be strictly limited.

The detailed investigation of the sea-floor is a necessary and somewhat neglected part of all coastal studies. This is largely because of the difficulties of the work, and of the expense involved. Off western Scotland a reliable and closely contoured map of the sea-floor throws much light on the former river systems and their connection with the sea lochs. The maps of the bottom deposits of the North Sea are helpful, but how little we know of the movement of material on the sea-floor! Are we justified in assuming that the great quantity of shingle which builds Blakeney Point originated in the Sheringham-Weybourne cliffs and the 'Eyes' of glacial material, some of which still exist near Salthouse, or must we assume, as seems essential for the much lesser amount of shingle at Scolt Head Island, an offshore source? In the immediate context these factors may seem mainly of theoretical importance but that such matters are of commercial and national significance will be suggested later.

In areas of dunes, shingle spits, and marsh, a factor of supreme importance in coastal studies is the interrelation of physiography and ecology. Although there are some very interesting cliff habitats that present problems of great scientific significance, they have little or no practical application. There is, on the other hand, room for much more controlled experimental work on plant habitats on low coasts.

Everyone is familiar with the growth of Marram grass (*Ammophila arenaria*) on dunes. The upward and lateral extent of the plant is closely allied with the sand supply. Many eroded dunes have been replanted with this grass, and if the sand supply is good, the grass grows rapidly and may soon heal a breach. In view of the

serious loss of dunes that occurred in north-eastern Norfolk and in Lincolnshire during the storm of January 31, 1953, and of the fact that there are always numerous places on a dune coast that could be replanted with advantage, it would seem worth while that experiments should be taken in hand to find if it is possible to produce a quicker growing plant ; or one tolerant of sea-water ; or even one that can cope more effectively with a poor and inadequate sand supply. If a line of dunes is severely attacked in a storm, the only hope, apart from expensive engineering work, is that there will be time for sufficient new growth to form before another storm. This usually implies a wide beach over which on-shore winds can blow and carry the sand inland. Unfortunately both north-eastern Norfolk and the central part of the Lincolnshire coast do not possess these ideal conditions, and dune growth is often slow. Any plant growth that can aid dune accumulation is to be welcomed, and careful experiments on *Ammophila*, *Agropyron*, and *Carex* suggest themselves. That marram can hybridise is proved by the occurrence of *Ammocalamagrostis baltica*, a cross between *Ammophila arenaria* and *Calamagrostis epigeios*.

Suaeda fruticosa is a shrubby plant that grows well at the head of a marsh just within the protecting ridges of shingle. Many years ago F. W. Oliver showed that as a shingle bank is gradually over rolled by waves, *Suaeda* may grow up through it, so that a time comes when the plant thrives on the crest, or even at the top of the front (seaward) slope of a shingle ridge. Here again there is room for experiment. Along a small part of the seaward side of Brancaster Golf Course, *Suaeda* has been planted with some success in an almost purely sandy habitat. In other parts of the world shingle beaches are often thickly covered with creeping plants. There is scope for investigation of the adoption and introduction of some of these plants into this country. Moreover, there is room for much experimental work in holding dunes and shingle ridges with shrubs and trees.

Nature herself has perhaps all too successfully demonstrated what can be done on salt marshes. In 1870 the rice grass, *Spartina townsendii*, first appeared on the south coast as a natural hybrid between *Spartina stricta* and the American species, *S. alterniflora*. It is common knowledge that the grass has spread rapidly in Poole Harbour, Southampton Water, and elsewhere. It is an admirable coloniser of soft and sloppy mud. Small patches planted at Blakeney have already spread to an alarming extent—and here, and in some other places, the grass has become a menace. But it is well suited for the reclamation of certain areas, and experiments were conducted on a few of the marshes outside the Essex sea-walls. The breaches in the Foulness walls and many other places made on January 31, 1953, make it more than worth while to do everything we can to accumulate in suitable localities mud and marsh in front of walls, and so cause the waves to lose as much as possible of their effective power before they reach a wall. The rates at which marshes grow upwards have been measured, and show how quickly accumulation can take place inside a shingle ridge and so, in part, help to stabilise it in position. Parts of the foreshore near Stiffkey, in Norfolk, seem to be admirably suited for long-term and big scale experiments covering marshes, dunes, and to some extent shingle. Ecological experiments may not be the direct concern of the geographer, but a joint physiographical and botanical approach to the problems involved is a great advantage. From my own experience I realise the enormous value of working on places like the marshlands of north Norfolk and Cardigan Bay with a good ecological botanist. The geographer is more directly concerned with the land forms and the source and supply of sand, shingle, or mud. To him, too, falls a study of wave incidence and the effect of waves of various types and from different directions on a shingle beach. The movement of beach material —a vital matter in any coastal area—is largely his concern. Since, too, his field observations are likely to have made him acquainted with some aspects of the rôle of the coastal vegetation he may be able to make an important contribution to the study of the whole problem.

I have spoken so far of natural protection to a coastline. The engineer is intimately

concerned with the problem of coast protection and erosion. Whilst he is perhaps primarily concerned with the erection of sea-walls, groynes, promenades, or harbour works, all of which should imply a full investigation of wave and tidal action, and of the travel of material on the beach and offshore, he also is concerned with ecology and the rôle of vegetation in foreshore protection. It is not uncommon for the engineer to be called in to deal with a problem and to have to act all too quickly. Some expensive harbour schemes have failed wholly or in part because full appreciation of the local condition of sand and shingle movements under the action of winds and waves from different directions was not obtainable. Nowadays the engineer often has access to a large laboratory in which he can set up on a small scale a model of the piece of coast he has to investigate. The geographer, too, has much to learn from such models and should take every opportunity of studying them and also—which is vitally important—comparing the results obtained from them with those provided by nature.

To appreciate the work of the engineer, all who are interested in coasts would do well to visit those parts of our east coast and the Zeeland area of Holland, which suffered so severely in the recent floods. The restoration of our defences was a work of major importance. The protection of life and property was the first consideration, but afterwards differences of opinion sometimes arose as to the right thing to do. The engineers had to act. In Holland where places like Schouwen were nearly all below low water mark, and solely protected by extensive walls, the problem was extremely severe. The original breaches were serious enough, but since the polders were so low, and since it was impossible to close the breaches at once, tidal scour added greatly to the problem, and consequently gaps a few hundreds of yards wide and often more than one hundred feet deep had to be closed by the construction of new banks well within the original, since not only has scour deepened the outlet, but it also led to the landward extension of broad and deep gullies.

The physical geographer would do well to contemplate these changes and apply the lessons so learnt. On a small scale the gap and gully cut at Titchwell, in north Norfolk, is of a similar nature. In Holland serious consideration is being given to the closing of the distributaries of the Rhine between Walcheren and the Hook. Such a great operation involves long and careful study, and although a problem, such as that posed by the actual closing of these waterways, demands great engineering skill, the geographer may be able to contribute to it by a full consideration from his point of view of the travel of beach material, by a study of dune growth, by considering as fully as he can the effects of wind and wave, and in other ways. I do not for a moment suggest that all these and many other factors will not be considered in great detail by the engineers, butt hat the geographer, who is concerned with the natural coast and the influences working upon it, and also used to considering an area as a regional unit, as a result of his experience, could bring to bear a point of view of considerable value. On a smaller scale the student of coastal forms, as distinct from the engineer, may well have a point of view to offer in a discussion about, for example, the closing of the Essex creeks. In short, geographers with local knowledge of any parts of the coast, in this or any other country that is likely to suffer major alteration at the hands of man, would do well to study it thoroughly so that their findings can be available before final action is taken.

Considerations of this sort provoke the question : How much do we know of the origin of even simple shore forms ? No one has completely explained why a shingle spit may alternate between longitudinal growth and the sending out of recurved or lateral ridges. In Orkney and Shetland and also in the Outer Hebrides there are numerous examples of what are often called bay-head bars, mid-bay bars, and bay-mouth bars. They have never been fully investigated and from what I have seen of them their origin is by no means simple. A straight-forward description of their characteristics would help a great deal. Some are obviously related to storm action ; others seem to bear, at first sight, little relation to storm activity

because they occur in relatively sheltered places. Some are double, e.g. those between Fora Ness and Mainland (Shetland); others—among them some of the ayres of Orkney—are perhaps partly of the nature of short offshore bars driven back to the main coast of an island. Many of the Hebridean boulder beaches are apparently the direct result of the Atlantic waves. But for all these features, each fairly simple in itself, there is no overall and convincing explanation. The problem is a physical one and essentially geographical, and in Shetland, at any rate, may well be connected with recent and even present downward movement of the land relative to the sea. The significance of submerged modern peats and the beach and dune systems of North and South Uist, Benbecula, and Harris need further investigation—to say nothing of the ecological problems of the Machair itself. The volume of literature on places like Dungeness and Chesil is considerable—but it is still uncertain how either of these features evolved. Exactly how does shingle move across Rye Bay? These and other examples are well known and are given publicity in many textbooks, but the uncertainty as to the manner of their formation and of many other well-known cases is not always properly appreciated.

So far my remarks have applied only to coastlines in our own islands—in fact to coastlines, allowing for differences in vegetation—in temperate latitudes. In the tropics we should have in mind at least two other factors. The first is the growth of coral, and the second the lithification of beaches. With the great ecological differences between a mangrove swamp and the low-growing plants characteristic of marshes in this country, there is no particular difference in function. Both the Norfolk marshes and the typical mangrove swamps alike collect mud and silt and so build up the coast. On the other hand, there is nothing in temperate climates to rival coral. From one point of view coral may act as a great sea-wall or as a promenade. A fringing reef around a coast protects it; still more so does a barrier reef which is not too far away from the coast. The Great Barrier Reefs of Australia are in places 50 or 80 miles or more from the shore, and since the prevalent winds also blow somewhat obliquely across the broad channel, the erosive power of the waves is often great. One of the most striking features about the Queensland coast is the presence of cliffs where the protecting reefs are far distant, and their absence where the reefs draw near to the coast. An interesting parallel to this occurs, on a small scale, in Shetland. For some miles south of Scalloway the coast is protected by West Burra, and there are no cliffs. South of the island of West Burra the steep slopes are cut into fine cliffs.

Lithification of beach sands takes place as the result of the precipitation of carbonate of lime from solution in seawater. The incidence of the process is not well understood, since around many small coral cays both in Australia and Jamaica, the beaches are not lithified all round the island, but usually (not invariably) on their windward sides only. But once lithification has taken place, the beach becomes hard and resistant, and many examples exist where a cay has, in a storm, been swept from inside its protecting lithified frame. I have never seen an account of the way in which this process is related to defence works, but in certain circumstances it should be a great help. The formation of beach rock seems to be almost confined within the upper and lower tidal limits, and also to tropical or subtropical waters. In colder waters, and in our own in the glacial period, the effect of ice as a mechanical agent is often important. Ridges simulating beach ridges, may be formed by it.

The tidal range on a beach, in whatever part of the world it may be situated, is of the utmost importance. It is a commonplace that at the time of high water bigger waves can approach the shore. These in their turn can be more effective erosion agents, and as a result of so much more water being driven on to a shore, strong undertow or some form of return currents must be set up which are capable of carrying away great amounts of material. It is certainly in storms that most damage is done to coasts. Careful note must also be taken of the incidence of a storm to the coast. A direct on-shore attack is liable to be more destructive than any other. But it follows that the greater likelihood of

damage will occur if the storm coincides with the top of a spring tide.

The word surge, as applied to tidal and meteorological phenomena, was first used after the severe flooding which took place in the London area on January 6–7, 1928, when during a high spring tide and a great outflow of the Thames there was overtopping of the walls and embankments in London itself. Since that date surges have been studied, and a surge may be defined as a meteorological tide, due entirely to wind and storm action, superimposed on the purely astronomical tide. The latter can be estimated months ahead with great precision. It is wind and local conditions which produce the variations. The surge of January 31, 1953, travelled down the east coast of England, and reached a height of nine feet. Fortunately the peak missed the top of the tide by an average of three hours, and even more significant was the fact that these particular tides, although springs, were low springs. If the surge had occurred a fortnight either earlier or later the results would have been far worse. It may also be remarked that January, 1953, was dry, and so the outflow of the rivers was low, otherwise the damming back by the tide of the river flow would have led to severe freshwater flooding. However, even with all these restraining circumstances, the surge lifted the water level along our east coast six or more feet above that of even high tides. Moreover, it was accompanied by severe wave action. Hence, in places cliffs were attacked many feet above that normal to storms, and for the same reason dunes were cut away. Amongst many others, two points are noteworthy. The storm winds were from north-north-west; hence, except locally, even soft cliffs were not severely eroded in Suffolk, whereas greater erosion took place on the north Norfolk coast. Although this statement is true, accurate measurements made by W. W. Williams at Covehithe showed that 90 to 40 feet were eroded from cliffs varying in height from 12 to 40 feet. But soft cliffs near the Coastguard Station at Dunwich were hardly touched !

The final rise of the surge occurred rather suddenly, and it is interesting to note that even small dunes were in places undamaged although completely inundated. On Holkham beach, for example, some low lines of dunes have grown since the war. Mapping completed in the summer of 1953 showed that in general form and position they had not altered since 1951 when they were also mapped. On the other hand, where sea-walls were overtopped, and the beach in front was narrow and none too high, damage was severe. The contrast between the Mablethorpe–Ingoldmells part of Lincolnshire and the parts to north and south of that stretch was indeed striking.

The way in which the surge behaved along the east coast emphasises the importance of the varying nature of the coast, and, consequently, of the great significance to the geographer and others of knowing as much of the coast as possible. Knowledge of only a few parts of the coast from personal experience, together with that gained from small scale maps may be dangerous indeed. The study of the one-inch maps of the Ordnance and Geological Surveys is of the utmost value, but is far better if to it is added a personal knowledge of large tracts of the coast. Only in that way can a proper appreciation of the setting be obtained, although little may be understood of local detail. Each mile is worth special study, and since each mile must to some extent depend upon each adjacent mile, so an intensive study of as much as possible is desirable. Textbooks are bound to generalise, and in coastal studies, as in many others, it is difficult for them to remain entirely up to date. Too often the coast is regarded two-dimensionally as far as the factors working on it are concerned. The tidal range, to say nothing of atmospheric conditions and wave action, the upward growth of marshes, and the possible scouring of the offshore bottom, and secular movements of land- or sea-level or of both, adds a very significant third dimension.

At this point I should like to revert to the relationship of geographers and engineers in coastal matters. Both are often interested in the same phenomena—including the drift of beach material and the movements of offshore sediments. Places like Southport and Yarmouth continue to accumulate wide beaches, whereas Withernsea and several other resorts have great difficulty in retaining

adequate beaches. Erosion attacks some cliffs with vigour, and each year several feet are cut away from Holderness and the soft cliffs of Suffolk and Norfolk. In contrast, Brighton can protect its eastern cliffs by promenades and walls, although erosion is also slower there. The engineer, just as the geographer, is vitally interested in low coasts. Can he hold a shingle ridge, cause dunes to grow, or bring about accumulation of mud and silt? I suppose the main difference in outlook between the engineer and the geographer on the coast, is that the former is concerned mainly with defence works which involve considerable expense and special technical knowledge. Thus the purely economic aspect matters a great deal. The geographer is free from this tie and is more interested in purely natural development. The point I am anxious to make, however, is that I think a close liaison would often be of benefit to both, especially if a large natural experimental area on the coast itself could be obtained. As I have already said I think the north Norfolk coast possesses many advantages in this respect. The serious depletion of beach material from the Lincolnshire coast between Mablethorpe and Skegness has been investigated by Dr. King and Mr. Barnes. Their report should be of great value to the engineers who are concerned with defences on this coast. Experiments with groynes may be well worth while, whereas the setting up of a groyne system designed for a long period may be fraught with danger.

A slight digression may be permissible here. To study the effect of waves, currents, and other factors on a stretch of coast takes time. Moreover, since, especially on our open coast, each part depends to some extent on the part next to it, it should follow that a comprehensive study of the problems of any given place involves a study of the adjoining parts. Holding up material in a particular locality may well have reactions in places five, ten, or even more miles away. Unfortunately practical considerations do not always allow such extensive studies to be made before some immediate action has to be taken at the place which needs attention. The present system of dividing the coast between River Boards and Coast Protection Authorities, with the consultation concerning adjacent areas that is also required before any action is decided upon, is sound and sufficient—the more so when a particularly competent engineer who really knows the coast is attached to one of the Boards. Some of the most successful defences have been built by engineers who are not only thoroughly competent, but who also know and love the coast on which they are working.

It is more than doubtful if any extended and comprehensive central authority could be as effective. Whilst it may be possible to envisage a national coastal authority, I am extremely doubtful if its work would be worth while because in the end those who know local conditions must play the effective rôle.

It is, perhaps, a pity that we did not have a surge like that of last year soon after the end of the First World War. At that time our coastline, from the human point of view, was in a very different condition from what it is now. The surge did great damage to life and property in many places, especially in the central part of the Lincolnshire coast, at Jaywick, and at Canvey Island. These three places lie within sea-walls, and are themselves below the level of high-water mark. They are therefore potentially liable to a certain amount of flooding at any time if a sea-wall were to collapse at a high water. As it was, the surge here and elsewhere overtopped the walls, by some feet in places, washed away the soft back parts, and so led to the formation of great breaches, thus inundating extensive areas and causing serious loss of life and of property. But should the property have been in such places? We have allowed building in many dangerous sites, and, after all, Jaywick and Canvey are but reclaimed salt marshes and unsuitable for development, nearly all of which took place between the wars. In short, had the surge of January, 1953, occurred in, say, January, 1923, should we have learned how to treat our coastline properly and more or less in time? At that period bungalows were comparatively rare, and many unsightly lines of cliff-top houses did not exist. Between the wars the attraction of our coast rightly increased, but unfortunately reasonable control of building was not always exercised. Many miles of good

sandy beaches, cliffs, and salt-marsh areas were disfigured by shacks and tawdry buildings. The building of holiday camps has saved much, and there is no doubt that, if permanent camps and properly grouped buildings had been encouraged earlier at the expense of the individual bungalow, we should have a far less spoiled coast than is the case. Anyone who will travel the coast from Southampon Water to Flamborough Head or from Llandudno to the Dee, will not fail to be impressed by the hideousness of much of our seaside development.

Much of our coast is, from the amenity point of view, seriously blemished by industrial activity of one kind and another. This is often unavoidable, but nevertheless the recent spread of industry to the fine sandy forelands of South Wales is unfortunate. Parts of Fife, the north-east coast of England, South Wales, and Cumberland are amongst the most devastated industrial coastal areas. In a minor way the former tin and copper mining in Cornwall left considerable scars.

The natural growth of seaside towns, the expansion of harbour facilities, and similar activities also extend along many miles of coast. If we study a map of England and Wales we shall find it difficult to pick out stretches of coast of more than five or ten miles which are to all intents and purposes unaffected by other than ordinary rural development. Even if the actual coastline is clear, there is a likelihood that misplaced and possibly unsightly buildings mar the views along the cliff tops. Let us, however, remember that industry has as much right to the coast as has urban development or what may be loosely termed amenity. What matters is to keep a just proportion between these demands, and also wherever any 'unnatural' development takes place, let us try to make it tone in with its surroundings as much as possible.

A striking contrast occurs in most of Scotland. North of the Highland boundary fault nearly the whole coast is entirely unspoiled. The bleaker and wetter climate and the generally unproductive nature of the country largely account for this, but it is nevertheless salutary for all who are concerned in planning—a word none of us minds using actively, but nearly all of us object to in the passive—to visit this coast. There are differences both too many and too obvious to mention to this audience between the Highland and English coastlines, but a visit to the north reminds one of the former solitude of the English coast, and should also make us realise how, even with our great population, we could have preserved, far better than we have, one of the greatest natural assets in our possession.

It is an unfortunate comment on the times we live in that several parts of our coastline are only open at certain very restricted periods to the public on account of military activity. There is both a good and a bad side to this. In general the restricted areas, in so far as the coast itself is concerned, are protected from any other kind of development, but under present legislation any such development would have been improbable. Whilst we all deplore it, none of us can take objection to the setting aside of areas both large and small for military training. What is regrettable is that some of these areas are parts of our coastline which can ill be spared. The use of three separate, even if limited, parts of the coast between Sheringham and Hunstanton seems somewhat out of proportion. Is it not possible to make more use of some remote part of Scotland? The Lulworth district of Dorset is also seriously spoiled, and there are other examples. I am well aware of war-time necessities and also of the post-war uncertainties, but it is difficult to divest oneself of the thought that convenience of permanent camp personnel rather than reasonable access to the coast for the country as a whole, or even of sheer military necessity, has been one at least of the major factors in the siting of some of these camps.

Geographers, no matter what their individual specialisms may be, are, or should be, very much concerned with coasts, and especially with our own coast since it presents so many interesting enquiries to pursue, and, let me emphasise the point, enquiries which are emphatically in their own province. The study of shore processes both in the field and, as far as practicable, in the laboratory afford wide scope, and the more they are

pursued the less inclined will the researcher be to generalise. So much of our knowledge of these processes is arbitrary. We need information about the movement of material both on and off shore along many miles of our coast ; we want to know how it varies with different winds and waves, and how it may vary with tides and tidal streams and with any combinations of these factors. There is wide scope for the investigation of sediments and of the processes of sedimentation, especially, for example, on the marshland of Norfolk. I have spoken already of the many ways in which the plant geographer and ecologist can contribute to an understanding of coastal phenomena. They, too, may well be able to help greatly in restricting erosion by experimenting far more widely and boldly than is done at present. The human geographer has more than enough to study—from the siting of new factories or other industrial undertakings to the cottage gardens on some small patch of raised beach in the Highlands, or from the small harbour and settlements such as Gardenstown, Cullipool, or Clovelly to the building of railways and roads along many miles of raised beaches in North Britain. Moreover, the greater the understanding of the economic geographer of the natural value of the coast the better. I believe, too, that the historical geographer will find abundant material for study—from the wider aspects of strategy and politics in the selection of ports and naval bases to the detailed, academic, but extremely interesting, minutiae of the history and development of small towns and havens, coastal castles, or even lighthouses, and the information that those studies may throw directly or indirectly on the purely physical evolution of coastal scenery. On a far larger scale much work has yet to be done before we can understand why we have a coast at all ! We do not know *precisely* how and when we were separated from the Continent across the Straits of Dover. Still less do we know of the origin of the coasts of the remainder of our islands. Do cliffs on our east coast represent simply the present stage of cutting back by the waves of a sea filling a gradually subsiding basin ? Submarine contours suggest many former rivers around the coast of Scotland —but when were Orkney and Shetland and The Long Island separated from the mainland ? Why are raised beaches absent from Shetland, somewhat rare in Orkney and Caithness, but well developed south of Helmsdale ? Was the last land connection between Ireland and Great Britain from Donegal to Islay and Jura ? Many matters of this sort, absolutely fundamental in their nature, remain largely unanswered. The geographer is much concerned with them, but here his work and interests are closely related to, and pass into the field of, the stratigrapher and perhaps of the geodesist. I remarked earlier on the importance of knowing the sea-bottom contours—in this wider study of the origin of our islands and of what I may call our primitive coast the study of the conformation and structure of the adjacent sea-floor is essential.

The more I have seen of our own and of other coasts the more I have been impressed with the amount of work there is to be done on them, and I am convinced that much of that work is far and away best attempted by geographers. In saying this I do not necessarily mean that the geographer's rôle is cut and dried ; indeed rather the reverse. Nor do I imply that the geographer is omnipotent —in fact, far from it. But by our very training we should be able to visualise the locality, the district or, if you prefer it, the region, and try to appreciate the whole and the interworkings of its various parts. Whether our inclination is to the physical or to the human side of our subject does not matter—there is work for all, and what is of extreme importance, work for exponents of different subjects. We, as geographers, would fail badly if we did not ask from, and give fully to, our colleagues in ecology, geology, engineering, hydrology, and other subjects. Whilst we can contribute an essential part to the study and understanding of a problem, my own experience has given me the firm belief that there are many problems associated with the coast which can only be unravelled by co-operative effort. When all is said and done no one person has the ability to interpret the whole of the natural landscape, nor is it the preserve of only one science.

THE CHANGING STRUCTURE OF THE BRITISH ECONOMY

Address by

PROF. E. A. G. ROBINSON

PRESIDENT OF SECTION F

THE dynamic development of the economy of any nation derives from a two-fold process of adjustment : it derives in part from a constantly continuing adjustment to technological progress ; in part from adjustment to the changes in the quantities and distribution of resources of materials and skills possessed both by the particular nation and by other nations. There emerges from these adjustments superimposed upon its own past history the structure of that particular economy at that moment of time. The structure often reflects only imperfectly the needs of the moment, for economic structures change slowly, and it is only in a period of relatively slow change that the actual structure and the ideal structure are likely to be identical. It is the changes in the structure of the United Kingdom's economy during the past two or three centuries, and particularly during the past hundred years or so, that I wish to review very briefly today. For we live in a period when large and more than usually rapid structural adjustment is both needed and taking place. I believe that the problems that confront us today are more easily understood if we see them in the perspective of the changes of structure that have occurred in the past.

The essential features of any nation's economic structure are made up of the pattern of employment and activity, the pattern of export and import, and the pattern of consumption. These patterns are in their turns related to and derived from the available resources, both natural and, as in the case of skills, acquired and inherited from an industrial tradition.

It is normal to think of the economic structure of any one nation as greatly influenced by the abundance of certain resources and by deficiencies of others. At the beginning of the eighteenth century it was still true to say that the pattern of the British economy derived in large measure from our ability to export the indigenous products of which we had a surplus. Of the £3·6 m. or thereabouts of exports of produce and manufactures of the United Kingdom, for the year 1688, about £2 m. represented woollen manufactures, and most of the rest represented exports of coal, lead, tin and other indigenous products. It was quite late in the nineteenth century that there developed the complexities of our present structure, whereby we import not only the materials required for our exporting industries, but also the greater part of the food needed to maintain our population.

May I begin, then, with the structure of 1688, when Gregory King, Lancaster Herald of Arms, made the study of *The State and Condition of England* which is the starting point for all modern macro-economic investigation ? England, with a population which King puts at 5,500,000, had an income which, slightly adjusted to bring it into line with current definitions, may be put at about £50 m. Retained imports, which he estimates at £2·8 m., were approximately 5½%, therefore, of national income.

Of a total of personal expenditure which amounts to about £46 m., approximately 51% was estimated to be spent on food and 25% on clothing. Of the food consumed, a very small proportion, mainly composed of tropical produce and wines, would seem to have been imported. Down to about 1730, England was an exporter

rather than importer of grain. The proportion of manpower resources devoted to agriculture cannot be estimated with any certainty from King's figures.[1] The proportion of gross national product estimated by him to be derived from agriculture is about 40%, and the proportion of manpower employed in agriculture must clearly have been at least of that order of magnitude, and probably considerably greater.[2]

Thus at the beginning of the eighteenth century we have a structure that is still predominantly self-sufficient, whose imports are primarily manufactures and the produce of other climates, whose exports are based on the utilisation of plentiful indigenous resources, and whose manpower is primarily devoted to agriculture, and which provided a level of real consumption per head a little under half of that of 1925–34.

I wish that one could find an opportunity for comparisons in the middle of the eighteenth century. It is sad to have to say that the century of English economic history which is of greatest potential interest, in that it saw the largely unexplained acceleration from the slower processes of earlier centuries to the rapid cumulative progress of the past two hundred years, is also the century that is completely lacking in estimates of national income and of the other main bench-marks to which one can refer such other statistical information as we possess.[3]

The earliest date after 1688 at which we can again attempt to see the main features of our economic structure is around 1800–1812. The various estimates of Beeke, Levi, Colquhoun, none of them individually wholly complete or internally consistent, nonetheless give us a sufficient basis from which to calculate rather imprecisely the essential elements of the contemporary structure. The national income of the United Kingdom was probably about £400 millions at prices of 1812. Retained imports, affected by war, were fluctuating in the range at current prices of £40 m. to £80 m.,[4] with a mean at prices of 1812 of about £49 m. Thus the import ratio would appear to have become something of the order of 12%. That is about the figure also for 1820, and it is from about 1820 that the data begin to accumulate from which one can see the main changes in the essential features of the structure in a peace-time economy.

In the years between 1820 and 1830, then, the structure of the United Kingdom had already begun to alter in many essentials. Exports were composed now only to the extent of about 50% of indigenous products of the United Kingdom and manufactures made from them; woollen manufactures accounted for a little under 12% and coal for about 1%; cotton manufactures accounted for about 48% of the whole, textile manufactures of all sorts to about 67% and manufactures of all kinds to about 90%. On the other side of the account, in consequence, imports of materials were also about 60% of the whole, out of which textiles materials, mainly cotton, accounted for about 24%. Retained imports of food, drink and tobacco were equal to about £20 m. at actual values and may be compared with

[1] If one is prepared to regard his categories of 'labouring people and outservants' and 'cottagers and paupers' as primarily agricultural the proportion would be as high as 75%. If, as seems likely, the first of these categories is intended to include most of the unskilled labour in the towns (artisans and handicrafts are elsewhere included), and both of them a certain amount of domestic industry, the proportion might be nearer to 65%.

[2] In 1691 Sir William Petty commented 'There is much more to be gained by Manufacture than Husbandry; and by Merchandise than Manufacture' (quoted by C. Clark, *Conditions of Economic Progress*, 2nd Edition, p. 395).

[3] I believe that it is worth trying even now to see whether this deficiency cannot be remedied. We have greatly developed in the past twenty-five years the arts of establishing the orders of magnitude of the national incomes of backward countries by the ingenious use of many individual clues to establish the totals of incomes, production and expenditure and to permit estimates of each of these to be employed as a cross check on the others. It is by no means clear that sufficient data do not exist to enable us to do much the same for the eighteenth and early nineteenth century, basing oneself in part in the latter case upon some of the rudimentary and internally inconsistent estimates made at the time. I am delighted that Miss Deane of the Department of Applied Economics at Cambridge is exploring these possibilities in connection with Dr. Simon Kuznets' big international project for the measurement of economic progress.

[4] As revalued from official values to current values by Professor A. H. Imlah, 'Real Values in British Foreign Trade, 1798–1853,' *Journal of Economic History*, November 1948, pp. 148–9.

a production of about £120 m. in agriculture ; thus foreign supplies of food represented something of the order of one-seventh of total supplies. The Census of 1821 gives us some limited help in seeing how the occupied population was employed : in England and Wales about 34% (a figure close to that of France before the last war) are reported as employed in agriculture and fishing ; about 46·5% were engaged in trade, manufactures and handicraft ; the remaining 19·5% were accounted for by all other forms of services and employments. Thus the economy had already moved a long way from the pattern of 1698. It had no longer in anything like the same degree as then the features that we now associate with an 'under-developed country' : the relatively large proportion of population engaged in agriculture or engaged in the trading and handicrafts associated with it ; the chief imports composed of manufactures and chief exports of primary products (that feature had largely disappeared before 1750). But the structure was still a long way from that which developed later in the century.

By 1850 we are beginning to see more clearly the features of what most of us were brought up to regard as the timeless characteristics of the British economy. The import-ratio had now risen to about 18%. Exports are composed to the extent of over 90% of manufactures ; textiles still predominate and account for about 63% of total exports ; woollens 14%, cottons 40%, and the rest mainly linen. Imports are still largely composed of raw materials for the textile industries, which account for 32% of the whole. Food imports are now increasing, and are about 34% of the whole, with grain imports up to 10% as against 4% in the 1820's. Exports are to a significantly smaller extent than in earlier years based on indigenous supplies. Of total exports of British products and manufactures, between 2% and 3% are accounted for by coal ; wool from our own sheep provided now about 70% of total supplies as against 95% half a century earlier, and we are beginning to be dependent on imported supplies of other materials also which had earlier been derived from home sources. The distribution of manpower reflects these and other changes. The proportion of occupied population engaged in agriculture has fallen to somewhere about 21%[1] ; better Census classifications, introduced in 1841 and which persisted almost unchanged until 1911, enable one to see the employment of the rest of the occupied population in more detail ; manufacturing in total employs just under 33% of the whole—a figure which was not exceeded until 1951—and of these over 22% are accounted for by textiles and clothing, a percentage very nearly three times as high as that of 1951.

Between 1850 and 1870, these features of high specialisation, a high import ratio, great dependence on manufactured exports, and especially on textile exports, diminishing dependence on indigenous raw materials and diminishing dependence of home food supplies all took a much more pronounced form. Britain was very rapidly adjusting her economy to a world market, made accessible by new and improved forms of transport.

But it is important to remember how immensely different that world was from the world of today. In 1850, so far as I can judge from some brave guessing to fill in gaps in the calculations of Mr. Colin Clark which cover the majority of the bigger countries, the real income of the world was between a fifth and a quarter of what it was in 1937. It has grown since 1850 partly as the result of unprecedented population growths, partly as the result of the great growth of productivity. But in the process of that growth it has come to be very differently distributed. Of the world income of 1937, about 29% was that of the North America countries and 4% that of Central and South America ; Western Europe accounted for about 31% and Eastern Europe for about 11% ; the Far East for only about 20% ; all other areas including Africa, Australasia, the Middle East, for about 5%. Of the world income of 1850, the largest share almost certainly went to the Far East, which would appear to have enjoyed something

[1] There are considerable difficulties in interpreting the 1841 and 1851 Censuses in this respect, and particularly in judging from them the trend between those two years. See Charles Booth, 'Occupations of the People of the United Kingdom, 1801–81,' *Journal of the Royal Statistical Society*, vol. xlix, p. 325.

over 40% of the whole; North America had a real income comparable with that of Africa today, and accounted for only about 10% of world income, with Central and South America representing about another 3%; Western Europe had a slightly smaller share than now with about 29%; Eastern Europe was rather more important with about 14%; the Middle East had about the same share as today; Australasia and Africa were almost completely negligible.

It was to the resources and requirements of that very different world that Britain was adjusting herself at the middle of the nineteenth century—a world in which the predominantly agricultural countries of new settlement were anxious to trade their primary products for manufactures; a world in which about 65% of the occupied population of the United States was still engaged in agriculture; a world in which Britain was probably responsible for over 40% of the world's total output of manufactures.

It was between 1850 and 1870 that the nineteenth-century structure reached its more complete form. By 1870 the ratio of retained imports to net national income at factor cost had risen to about 28%—a figure surpassed only for a short time in the 1880's when it reached for a few years a level of about 33%. The pattern of imports was changing. About 35% of all imports were now food, of which grain was 9%, and about 50% raw materials; manufactures accounted for about 15%. But the main sources of food were still in our own agriculture. For the year 1867, according to estimates by James Caird,[1] home agricultural produce accounted for about 82% of total supplies of the main staple foodstuffs, and probably about three-quarters of the whole when allowance is made for imports of sugar and other tropical produce. Exports of textiles accounted for about 56% of the whole; cotton now 35%, wool 13%. But exports of metals and metal manufactures were beginning to mount; they now represented 21% of the whole. At the same time the industrial distribution of the occupied population increasingly reflected this changing emphasis. Between 1851 and 1871 the proportion engaged in manufacturing of all kinds slightly declined, if one may trust the figures, from about 33% almost to 30%; but within the total metal manufactures rose from 6% to nearly 7½%. The proportion in agriculture fell heavily—from 21½% to a little over 14%, while the proportions engaged in transport and commerce rose from just under 11% to about 13½%.

Between 1870 and 1913 the overall ratio of retained imports to net national income remained relatively steady, rising from the 28% of 1870 up to a peak of 33% in the 1880's and then falling back to slightly lower figures in the 1890's. But in all else the change continued to be rapid. The redistribution of manpower continued. Agriculture, which had given employment to over 14% of the occupied population in 1870, was down to 8½% at the turn of the century and to about 8% by 1913. The total of manufacturing retained its surprising stability around 29–30% of the whole, but within this total the redistribution continued, away from textiles, which fell from about 18% in 1870 to about 13½% by 1913, and towards metals and engineering, which rose over the same period from about 7½% to about 10% of the total. And the trend towards transport, commerce and distribution continued; this group rose from 13½% in 1870 to about 20% in 1913.

Parallel with this redistribution, and springing from the same causes was a change in the pattern of imports. At 1850 and again at 1870 food imports had been close to 35% of the whole; they rose to about 42% of the whole at the turn of the century and for the first time were larger than raw material imports; the share of the latter increased again by 1913, but it remained nearer to 40% than to the 50% of 1870 or the 60% of 1850.

It is not easy to measure precisely the degree of dependence on imported food at the culmination of the nineteenth-century development, much as one would like to do so. As regards grain of all types, we imported about 50% of our total supplies

[1] James Caird, 'On the Agricultural Statistics of the United Kingdom,' *Journal of the Royal Statistical Society*, vol. xxxi, pp. 127–45. The estimates are concerned only with such basic foodstuffs as grain, beef and mutton, butter and cheese, potatoes, and take no account of tropical imports, fruit, tea, coffee and the like.

at the turn of the century[1] and 55% in 1913, as compared with about 70% in 1936–8. As regards meat, we imported about 40% in 1900, and again in 1913, rising to just under 60% in 1936–8. Overall, and including products in which we were much nearer to self-sufficiency, I would guess (and it can be no more than a guess) that home agriculture provided about 50% of our food in 1900, about 45% in 1913,[2] about 35% in 1936–8, with a return to about 50% in the last few years.

Thus by 1913 the structure based on a high specialisation in manufacture, on imported materials and on imported food had reached its fulfilment. At the 1911 Census, agriculture accounted for only 8·1% of the occupied population as compared with 21·6% seventy years earlier. Manufacturing in total gave employment to just about the same proportion of the whole. The proportion of the occupied population engaged in transport, commerce and distribution had grown by about as much as that in agriculture had declined.

It was a specialisation no longer based on indigenous resources. Coal, it is true, still accounted for 10% of our exports, and woollens for 7%. But the woollens were increasingly produced from imported wool; it had formed 50% of the available supply[3] in 1857–63; it now formed about 84%. It was a specialisation still based predominantly on textiles. And new textile industries were beginning to grow all over the world—not only in North America, but in Japan, India, China, and later in South America. Thus the structure became insecure almost at the moment that it was completed. The British share of the world total of production of manufactures inevitably declined as new industries grew up in other countries; it is almost a tautology to say so. Whereas the British share had been probably appreciably above 40% in 1850 it was down to about 32% in 1870, about 20% at the turn of the century and about 14% in 1913.[4]

The structure of 1913 was one of very considerable immediate advantage to Britain, but it was inherently fragile. No one country could possibly hope to retain indefinitely, however great its technical leadership, the share of world manufacturing that fell to us in the middle of the nineteenth century, or the share of world trade in manufactures that went with it. The share of that trade held by Britain fell from about 40% in 1870 to about 30% at the turn of the century and 27% in 1913. It was a declining share, however, in a rapidly growing total. Thus the problem of paying for imports was never acute before 1913; normal imports had never been increased to the full capacity to pay for them, and invisible earnings left a handsome investible surplus in most years. But what I wish, nevertheless, to emphasise is that, quite apart from wars, our 1913 structure was based on an advantage which was temporary and not permanent: that the incentives to shift resources to industry and reduce primary production were already in operation in many countries on which we depended and might have been expected in the course of time to push us back from the position of extreme specialisation that suited us so admirably for the moment.

It was, moreover—and I do not think this is generally realised—a structure that was not based, or not in its extreme form, on a system of comparative advantages. It is not true to say that British productivity in manufacturing was higher in relation to British productivity in agriculture than that of many of the countries from which we were increasing our imports. The ratio of the value of production per head in industry to the value of production per head in agriculture has been more favourable to industry and less favourable to agriculture in the United States than in the United Kingdom certainly since 1870. The same is also true

[1] See R. F. Crawford, 'Notes on the Food Supply of the United Kingdom, Belgium, France and Germany,' *Journal of the Royal Statistical Society*, vol. lxii, pp. 597–629.

[2] See estimate for 1911 by Sir Henry Rew, 'The Nation's Food Supply,' *Journal of the Royal Statistical Society*, vol. lxxvi, pp. 98–105. The proportion, in terms of value, provided by home production was about 52% if tea, coffee, cocoa and sugar (of which there was then no home production) are excluded.

[3] See C. T. Saunders, 'Consumption of Raw Materials in the United Kingdom, 1851–1950,' *Journal of the Royal Statistical Society*, vol. cxv, pp. 336–8.

[4] See *Industrialisation and Foreign Trade*, League of Nations, 1945, p. 13.

of Canada if one may make plausible assumptions regarding Canadian productivity per head in agriculture for which contemporary estimates do not exist for the earlier years. It was only in Australasia and in South America that the comparative advantage lay throughout heavily on the side of agriculture.

The gain that we secured by specialisation derived from the terms of trade subsisting everywhere between manufactures and primary products. Over the greater part of the last eighty years there has been an advantage to any economy anywhere (with exceptions in Australasia and South America) which moved resources out of agriculture into industry and which exported manufactures and imported primary products. We in Britain could benefit through the fact that our technical lead, our financial resources and our geographical setting put us in the position to move more quickly than others.

The structure was inherently fragile in a second sense. Growing population to be nourished with food, growing industries to be nourished with materials, meant increasingly that marginal supplies must be derived from abroad. Food supplies by 1913 came, as I have said, to the extent of about 55% from abroad. At the same time we were coming to depend ever more and more on raw material imports, not only because of increased needs but also because of exhaustion of supplies. In 1857–63 we could supply all our needs of lead, 88% of needs of tin and 71% of our needs of copper. By 1907–13 we supplied only 11% of our lead, 21% of our tin and 1% of our copper. In 1857–63 we could depend on our own wool supplies for 58% of our needs ; in 1907–13 the figure was down to 22%, and similarly for flax.[1]

The years from 1919 to 1939 saw the working out of some of these forces that already existed before 1913. But to those already existing forces was added a new and more powerful force : the emergence of the United States during the 1914–18 war to a position of leadership in manufacture. Her share of world production of manufactures had first exceeded our own in the 1880's ; it was about two-and-a-half times our own by 1913 and remained about four times our own through most of the inter-war years. But the United States' share in foreign trade in manufactures was by no means proportionate to her production and remained between a quarter and a third of our own down to 1913. It increased to well above a half of our own in the years immediately following 1918, and rose to about four-fifths of our own during the later inter-war years. That dramatic growth of competition in our markets, and the subsequent and comparable growth of that of Japan, together with the loss of foreign assets during 1914–18, meant that the structure of 1913 required to be changed far more rapidly and more extensively than would otherwise have been necessary. I would in retrospect regard many of the travails of the 1930's as part of the inevitable and painful process of world adjustment and of our own adjustment to the growth of the United States and Japan.

The immediate effects were, however, obscured and their severity diminished by the favourable turn of the terms of trade in the 1930's[2] ; partly, it is true, the consequence of the lower prices of primary products relatively to manufactures to be expected in depression, but chiefly to be ascribed to the great revolutions in agriculture : mechanisation, new fertilisers, better strains, better transport. This favourable turn permitted us to survive the 1930's with appreciably less structural change than would otherwise have been required.

What were the chief changes in our structure during these years ? First, and largely a consequence of the change in the terms of trade, the ratio of the value of retained imports to net national income fell back from the 28% of 1913 to 20% in 1938. But with the aid of these improved terms of trade, and in the face of increasing difficulties, we retained the other main features of the 1913 structure. Manpower in agriculture continued to fall ; it was down to about 5·7% of the occupied population[3] by 1931 ; the proportion in

[1] See C. T. Saunders, *loc. cit.*

[2] The volume of imports which would have cost 100 units of exports in 1913 could be bought by an average of 70 units during the years 1931–9.

[3] On the classification of the period 1841–1911 ; the classification was slightly changed in 1921.

manufacturing remained about constant; that in commerce, finance and distribution continued to grow. But the main feature of these years was the decline of the textile industries and the growth of the metal industries. In 1851 there were 22% of the occupied population in the textile industries; by 1911 the figure was about 13½%; in 1931 it was below 11% and in 1951 under 8%. The metal industries employed 6% in 1851; they had reached nearly 10% in 1911, about 12% in 1931 and 17½% in 1951.

This change was naturally associated with the change in the character of our exports. Textiles had provided 63% in 1850 and 34% in 1913; by 1937 the figure was down to 24% and by 1951 to 19%. Metals and engineering products provided 18% in 1850; they were 27% in 1913; they were up to 35% by 1937 and to 49% by 1951.

Despite the strains of the 1930's we continued to increase our dependence on imported food. For our raw material supplies, with the possible exception of iron ores, we also became increasingly dependent on imports. Home supplies of wool and flax and of non-ferrous metals declined further.

This structure was, however, proving increasingly difficult to maintain. Despite exceptionally favourable terms of trade we were failing to pay our way on current account and balancing our payments with the help of drafts on our past foreign accumulations. Thus we entered the war with a structure very imperfectly adjusted to our current circumstances. The consequences of the war and, far more than those, the consequences of a reversal of the terms of trade left us with a structure very ill-adjusted to the world in which we now live.

May I look back for a moment on the century as a whole? In 1820 and right down to the 1870's we were scarcely as yet in any full sense a nation of shopkeepers. We were a nation of linen drapers—of John Gilpins rather than of William Whiteleys. We built our nineteenth-century economic structure and we multiplied the population of 1750 six-fold on the basis of our ability to sell cottons and woollens. We created by 1913 a nation which depended for about seven-eighths of its raw material supplies, apart from coal, and for over half of its food supplies on its continued ability to sell some kind of export into a world which was becoming less dependent on its textiles. Since the 1870's we had expanded metal and engineering exports and by 1913 they were a quarter of the total. But it is mainly in the period between the wars that we changed to being a nation dependent primarily on our ability to sell engineering goods, and in that field we have never enjoyed the sort of predominance we had for over a hundred years in textiles.

Before I go on to consider the forces that may during the next decade require further adjustment of our structure, may I pause to contrast the structure that has emerged from our history during the past two centuries with that which has emerged from the very different history of Western Germany. Western Germany has a population very similar to our own—49·0 millions as against 50·8 for the United Kingdom. The total areas of the two countries are almost identical. The agricultural areas are not widely different: Western Germany had 8·5 million hectares of arable in 1948–52 as against 7·4 million in the United Kingdom; the United Kingdom had 12·1 million hectares of permanent grass against Germany's 5·5 millions; Germany had 7·0 million hectares of forest as against our 1·5 millions. In normal peace-time conditions the real incomes per head have not been widely different; the United Kingdom probably had an advantage of some 15% as compared with the average of all Germany before the war, but the advantage over the more industrialised Western Germany was almost certainly less; recent calculations have shown a wider discrepancy for 1952, possibly as great as 20–25%, a discrepancy not unexpected and largely explicable in terms of the different consequences of the war.

In all else the structures of the two countries are radically different. In 1953 the import ratio for the United Kingdom was about 26%, that for Germany about 16%. It is not possible to carry back the German ratio in terms of anything like the present area. In 1937–8, when the United Kingdom ratio was about 20–22%, that for pre-war Germany as a whole, very

much larger and more self-sufficient, was about 7–8%. But the discrepancy had been much less in earlier years. In 1913, when the United Kingdom ratio was 28%, that for the contemporary area of Germany was about 24%.

The lower ratio and the lower income per head taken together mean that in 1953 German retained imports were about two-fifths British imports. Western Germany supports at present an industrial production about two-thirds of that of the United Kingdom, with imports of raw materials about one-third of the United Kingdom. She succeeds in doing this partly because of a much smaller activity in the textile industries, partly because of her almost complete dependence on her own timber resources ; these two sources of savings of imports accounted for two-thirds of the difference.

In food, the United Kingdom is now dependent on imports for about half her foodstuffs ; Western Germany for about one-fifth. But, with the imported food, the United Kingdom's real consumption per head was in 1952 some 15–20% higher than that of Germany.[1] The agricultural output of Western Germany is almost half as great again as that of the United Kingdom. She has achieved this by devoting to agriculture about 16% of her occupied male population as against 6% for the United Kingdom.

I do not wish to hold up the Western German economy as a model that we ought to copy. Both countries have much that they might learn from each other. The higher standard of life that we have enjoyed has in the past, I am convinced, derived in considerable measure from our greater specialisation and trade. I believe that Germany will be found as the years go by to be progressively moving again towards higher imports and higher exports, and in consequence becoming more directly competitive with the United Kingdom. But I do believe that one may learn both from the history of our own past and from a comparison with Germany that economic structures are not unchangeable and permanent for all time, but capable of great variation, and from time to time requiring great variation.

In what directions can we expect our own structure to change under the stimulus of the market forces and the economic pressures of a changing world ? My own view—and I claim for it no special authority—is that the structure which we created to such profit in the second half of the nineteenth century was fundamentally based on two factors. First, it was based on the possession by us of a leadership in manufacturing and a corresponding share of world trade in manufactures which have been diminishing progressively. I do not think that it is within our power, whatsoever energy and skill we may develop, to recapture anything comparable with that leadership. It was based on a concentration of discovery, of scientific knowledge and of practical expertise within a single country such as we can hardly imagine to be capable of repetition by any country. It was based on a slowness of the dispersal of knowledge and a lack of the technical background required for receptivity such as can hardly be re-created once it has disappeared. The degree of technical leadership of the United States today is in no sense comparable with that of the United Kingdom in the middle of the nineteenth century. And to that we must add the continuing improvements in means of transport, and particularly in land transport. I think the degree of difference in comparative productivity in different industries, apart from advantages of access to very advantageous raw material supplies, is diminishing as compared with the nineteenth-century differences and to an increasing extent the country in which manpower is four-fifths as productive as that in another in one industry is apt to be four-fifths as efficient in a very wide range of industries. Thus the opportunities for one country to secure and hold a large export trade by leadership and the advantages of specialisation seem to me to be less than they were.

Secondly, it was based on a relation between primary production and industry which in its extreme form I believe to have been temporary. There are signs that the forces which enabled us to benefit from the favourable terms of trade between

[1] See *An International Comparison of National Production and the Purchasing Power of Currencies*, by Milton Gilbert and Irving B. Kravis, Table 12.

manufacturing and primary production are working themselves out. · The shift of resources from primary production in North America has been on such a scale that United States agriculture has fallen from a share of 65% of the occupied population in 1850 to 29% in 1920 and to 16% in 1950. In North-Western Europe as a whole the proportion in agriculture has fallen from 26% in 1920 to 19% in 1950. In Asia and in South America also there has been a very marked increase in industrial activity. Thus it is not only the high levels of activity and consequent demand for primary products in recent years which, despite the continuance of the rapid upward trend of productivity in primary production, have reversed the trend of the terms of trade. The movement out of primary production and into industry consequent upon the powerful incentives of the 1930's has also, I think, contributed largely and more permanently. The ratio of the value of production per head in agriculture to the value of production per head in industry is now almost everywhere much more favourable to agriculture than it has been for a generation. The prosperity of British agriculture in the past few years has been not solely, or even predominantly, the result of subsidies and other political favours ; they have shared in the general recent prosperity of all primary producers everywhere.

For these reasons I believe that we are progressively moving into a world in which the advantages of our nineteenth-century structure in its extreme form will be less than in the past. I believe that by degrees, and under the pressures of the ordinary market forces we must expect and be prepared to adjust ourselves to an import ratio a little nearer to that of Germany than that we have today, and to the adjustment of the pattern of activity that may go with it. I have no desire to accelerate any necessary transition beyond the pace that changing world conditions may require ; least of all have I any desire to lead the United Kingdom into uneconomic autarky.

But I am anxious lest an undue conservatism may regard the structure of fifty years ago as not only the ideal of that period but also the ideal for the present, and so resist structural change at the pace at which it may be necessary. For, as I see it, conservatism in these things may lead us to seek to preserve an unsuitable structure in circumstances in which that can only be done, consistently with a balance of payments on current account, by taking measures of which the short-term effect is to reduce imports by reducing activity. By such measures we may well forfeit through reduced activity, and through reduced capital formation and reduced growth of productivity, considerably more than we gain at the margin by better international division of labour.

After such dramatic changes as have occurred as the result of the two wars of this century it is inevitable that our economy should require greater and more rapid change than if events have moved more smoothly and continuously. What we know least of all in the whole field of economics is the limits of practicable rates of change. We know how fast—or rather how slowly—the pattern has in fact changed over the past century. But we do not know just how fast it can be made to change by normal price incentives. These incentives will work in two ways ; modifying the pattern of consumption through the change of relative prices, and modifying the pattern of production through the change of incentives. But there can be no question that large structural change requires considerable time. It requires a change in the pattern of our fixed capital, of our skills and of the geographical and industrial distribution of the working population. In some cases the required structural change can be facilitated by a change in the pattern of imports and their ratio to national income as different types of resources become relatively more or less scarce. But any large scale resort to this solution requires the expansion of exports, and that may well be the critical factor in all our problems.

In war-time, when very rapid structural change is essential, we have been accustomed both to accelerate the change and to diminish its immediate necessity by the twin operations of controlling the use of resources and rationing forms of final consumption involving resources which are temporarily scarce. My own fear is that

events may continue to require us to make changes faster than a free and smoothly working economy can easily carry them through. It is for a willingness to face the necessities and implications of change when they come, and to foresee them and to be prepared for them before they come, that I would plead.

TABLE I

The Dependence of the British Economy on Imports

	Population of United Kingdom[1] (millions)	Approximate Ratio of Retained Imports to Net National Income at Factor Cost[2] %
1688	7·4	5½
1820	20·9	12
1850	27·4	18
1870	31·5	28
1880	34·9	33
1900	41·5	26
1913	45·8	28
1937	47·3	21
1951	50·2	32
1953	50·4	26

TABLE II

Percentage Distribution of the Occupied Population of England and Wales

	1821	1831	1951
Agriculture and Fishing	34·0	28·6	5
Trade, Manufactures and Handicraft	46·5	42·2	60
Others, not employed in above ..	19·5	29·2	35
	100	100	100

NOTE.—The figures for 1951 represent only a rough guess of the orders of magnitude of the similar categories today.

[1] For 1688 I have taken Gregory King's estimate, since I am using his national income and import estimates. For England (meaning, as is made clear, England and Wales) he estimates 5·5 millions; he says that 'Scotland and Ireland are . . . not ⅔ths of England in number of people.' He goes on to say that 'England, Scotland and Ireland together contain . . . somewhat more than 7 millions of people.' I have taken the mean between 7·0 millions and 7·7 millions. All other figures down to 1900 from the Census of the following year. Figures for isolated years since 1900 are mid-year estimates; the figures for 1937 and 1952 refer, as does the national income estimate, to the area now defined as the United Kingdom, *i.e.* excluding Eire; the figures for earlier years include Eire.

[2] Estimates of Net National Income at Factor Cost from A. R. Prest, 'National Income of the United Kingdom 1870–1946,' *Economic Journal*, March 1948, pp. 31–62, continued with help of official estimates. Earlier estimates of national income based on material from Colin Clark, *Conditions of Economic Progress*, and A. L. Bowley, *British Overseas Trade, 1700–1930*. Retained imports as revalued for earlier years by A. H. Imlah, *loc. cit.*

TABLE III

Percentage Distribution of the Occupied Population of England and Wales
1841–1951

	1841	1851	1861	1871	1881	1891	1901	1911	1911	1921	1931	1951
Agriculture and Fishing .	21·6	21·4	18·2	14·2	12·4	10·3	8·5	8·1	7·8	6·8	5·6	4·7
Manufacturing . . of which :	28·9	32·9	32·6	30·2	29·9	29·6	29·1	29·0	29·9	31·2	30·0	34·1
textiles and clothing .	19·0	22·2	20·6	18·1	17·2	16·4	14·1	13·4	13·7	11·4	10·7	7·7
metals, machinery, etc.	5·5	6·1	7·2	7·3	7·7	7·8	9·1	9·7	9·8	12·8	11·6	17·5
other manufacturing .	4·3	4·6	4·7	4·8	5·0	5·4	5·9	6·0	6·4	7·1	7·7	8·9
Manufacture of Food, Drink and Tobacco .	1·5	1·7	1·6	1·6	1·7	1·9	2·2	2·4	2·9	3·1	3·3	3·3
Mining and Quarrying .	3·3	3·9	4·2	4·2	4·6	5·0	5·5	6·3	6·9	7·5	6·4	4·4
Building, Decorating, Contracting . .	5·4	5·0	5·3	5·7	6·3	5·7	7·1	5·8	5·3	4·4	5·6	6·2
General Labourers . .	5·8	4·1	3·4	4·9	5·0	4·7	2·9	1·8	1·5			
Transport and Communications . . .	3·0	5·4	6·2	6·6	7·6	8·4	9·7	9·5	6·9	7·0	6·8	7·7
Commerce, Finance and Distribution . .	4·8	5·3	6·0	6·8	7·2	7·9	9·0	10·5	13·2	13·2	15·5	13·8
Personal Service . .	19·2	14·6	15·6	16·6	16·1	16·3	14·6	14·1	15·1	11·8	12·8	7·5
Central and Local Government Employment .	1·3	1·5	2·0	1·9	1·9	2·1	2·6	3·1	4·4	6·5	6·6	8·2
Residual . . .	5·2	4·1	4·9	7·3	7·3	8·2	8·9	9·3	6·0	8·4	7·5	10·2

NOTE.—The categories were substantially revised in 1921 ; the data for 1911 are available on both classifications. It will be seen that in few cases does the reclassification so seriously affect the figures as to obscure the general long-term trends, as shown by the older classification. But it is important to bear in mind that throughout the period the Census authorities and those making returns were improving their classification in minor respects and that by degrees such classes as general labourers were absorbed into their appropriate classifications without substantial change of the character of activity.

TABLE IV

Composition of United Kingdom Exports

	Manufactured Exports as % of total exports	Textile Exports as % of total exports	Metal and Engineering Exports as % of total exports	Coal Exports as % of total exports
1830	91	67	11	1
1850	93	63	18	2
1870	91	56	21	3
1890	86	43	25	7
1913	79	34	27	10
1937	78	24	35	7
1951	88	19	49	1

NOTE.—The definitions in this table correspond to the 1951 Board of Trade classifications. Textile exports do not include clothing.

TABLE V

Composition of United Kingdom Imports

	1820	1850	1870	1900	1913	1929	1933
Food and Livestock .	31	34	35	42	37	40·5	46
of which : Grain .	4	10	9	9	9	7	8
Raw Materials .	60	59	50	39	43	39·5	41
of which : Textile Materials	24	32	28	16	17	11	13
Finished Manufactured Goods . .	9	7	15	19	20	20	13
Total .	100	100	100	100	100	100	100

NOTE.—The analysis given above is derived from the work of Werner Schlote, *British Overseas Trade from 1700 to the 1930's* (translation by W. H. Chaloner and W. O. Henderson, Blackwell, 1952). He has attempted to distribute the recorded imports according to the Brussels international classification, and has revalued the imports for the purpose of calculating volumes and the prices of a series of base years and has then linked the separate series. While the latter process is irrelevant to my immediate problem I have used his material since it gives a convenient and constant set of classifications and enables me to go back to 1820. But a reader should be warned that the relative values of different products underlying this table are those that emerge from Schlote's work (*vide op. cit.* pp. 27–30), and which Schlote calls 'quantities at base year prices.' Since Schlote's analysis ends at 1933, the comparison cannot be extended into the post-war period.

TABLE VI

Ratio of Productivity in Agriculture to Productivity in Industry

	1870	1900	1913	1937
United Kingdom .	1·07	0·93	0·80	0·60
U.S.A. . .	0·52	0·44	0·39	0·39
Canada . .	0·96	0·63	0·43	0·27
Australia . .	..	..	1·39	1·41
New Zealand .	..	..	1·38	1·09
Argentine . .	..	..	..	2·12
Germany . .	..	..	0·60	0·48
France . .	2·0	..	0·77	0·59
Denmark . .	..	..	..	0·49

NOTE.—The above table is derived from estimates made by Mr. Colin Clark in *The Conditions of Economic Progress*. The underlying figures are calculated by him in his 'international units' and are therefore at the relative prices of 1925–34 and not at current relative prices. The output of agriculture is valued at world prices, and does not reflect the special advantages or protection available to domestic agricultural producers. The ratios are—it should be emphasised—the result of comparing *average* productivity in industry with *average* productivity in agriculture; the ratios of the *marginal* productivities may be appreciably different.

THE EFFECT OF NEW MATERIALS ON ENGINEERING PROGRESS

Address by

DR. WILLIS JACKSON, F.R.S.

PRESIDENT OF SECTION G

IN 1951 the President of Section G, Sir Claude Gibb, chose as the subject of his address "Two Thousand Years of Engineering" and this review of the past was followed by a look into the future when last year's President, Lord Dudley Gordon, attempted with much courage to suggest how the service and practice of engineering may develop in the next two thousand years. I propose to be much more modest in my choice of subject, at least in respect of its time scale, and to devote myself to a description of what has been happening in certain branches of engineering during the past 50 or so years, with particular reference to some of the materials which have attained engineering importance very recently and to the effect which they are exercising on engineering development.

The successive stages of man's progress are sometimes designated by the more common materials used by him. Thus it is said that mankind has progressed from the Stone Age, through the Bronze and Iron Ages to the Steel Age. It is not easy to think of a single word which could adequately designate the present era in these terms—even if this were a desirable designation—for alongside the continuing dominant use of steel for constructional purposes there has emerged a whole diversity of other materials, which, though relatively small if not minute in quantity, are of major importance for particular applications.

Man's discovery of the valuable mechanical properties of bronze and of steel was accidental in the scientific sense, for at that time there was no science on which it could have been based. The present situation is very different; the needs of man can no longer wait upon chance discovery, and engineering has passed beyond the stage when the desired results can be achieved by ingenuity alone. It has entered the stage when these results must be evolved logically by the human brain from the system of ideas which constitutes the intellectual edifice called natural science, and no aspect of it reveals this more clearly than the use of materials.

There must have been many definitions of engineering, and one of them might be that it is the economic exploitation by one technique or another of the properties of materials; from which it follows that progress in engineering is vitally dependent on the improvement of existing materials, and on the discovery and economic development of new ones.

This progress may occur in several, though usually inter-related, ways, of which I want to give a number of examples. Since the subject is a very extensive one and since I am myself an electrical engineer, I propose to choose these examples mainly from the field of electrical engineering.

The most obvious way in which progress can occur is by the steady improvement within a well-established technique of the materials employed in it.

One such technique in electrical engineering is the transmission of electrical power from one point to another by means of cables. This power may be for industrial purposes at a frequency of 50 cycles per second when the transmission voltage may reach a value of 275,000 volts and the power involved many tens of thousands of kilowatts; or it may be associated with the transmission of telegraph, telephone or television signals at

voltage and power levels which are relatively minute but at frequencies which in the case of television extend up to several million cycles per second. The construction of the cables employed for these two purposes is widely different due to the differing nature of the problems associated with high voltages in the first and with high frequencies in the second, but in both cases the major problems have been associated with the materials used for insulating the cable conductors from each other.

I will deal first with power cables. The last quarter of the nineteenth century saw the development of central power stations for generating electricity to be distributed to consumers in the surrounding countryside. Distribution was first carried out by means of copper conductors suspended overhead on porcelain or other insulators. However, the orderly Victorian mind reacted against the unattractive appearance of these overhead systems, and it was perhaps this circumstance which led to the early establishment of the power cable industry in this country. The conductors of a cable are surrounded by a material which serves the dual purpose of affording mechanical separation and support and of preventing current leakage from one to the other. Gutta-percha was already established as an insulant for submarine telegraph cables and this material was therefore tried for power cables together with vulcanised rubber and vulcanised bitumen.

These materials proved reasonably satisfactory so long as the transmission voltage was merely the 100 or so volts provided by the early direct current generators. But the need to distribute electricity over greater distances than this direct current system permitted required the use of higher transmission voltages. The requirement was met by the introduction of alternating current generators and the use of transformers to step-up the generator voltage to a much higher value for transmission. In 1885 Ferranti took the historic and courageous step of designing and laying a 10,000 volt A.C. cable from a new power station at Deptford to the Grosvenor Gallery in Bond Street, a distance of 5 to 6 miles, in which he employed two concentric copper tubes separated by lapped paper impregnated with ozokerite wax. The cable was made in lengths of 20 feet and in spite of the thousands of joints required this system operated satisfactorily for more than 40 years.

At the time of the First World War the accepted limit of voltage for underground power cable was 22,000 but attempts were being made to raise it to 33,000. After many failures and much experimenting this figure was established as safe, and the operating voltage in this country has since been raised to 66,000 and 132,000 until it has now reached 275,000 volts.

Yet throughout this extended development, and notwithstanding the very wide range of new synthetic insulating materials which have become available during the past 20 years, lapped paper has remained the basic constituent of the insulation, and indeed the impregnated paper cable is still the only form of cable which can be seriously considered for the very highest voltages. From some points of view this might appear surprising and be thought to suggest too conservative an attitude on the part of cable manufacturers, but it may also be regarded as a tribute to the ingenuity of these manufacturers in overcoming the limitations of paper by careful choice of its constituents and methods of processing ; by the steady improvement of the impregnating mineral oils which replaced wax around 1912 ; and by the introduction of new techniques such as the application of pressures up to 200 lb./sq. in. to the impregnating oil for the purpose of preventing ionisable voids within the insulating structure—the primary cause of breakdown. In consequence the permissible operating temperature has been doubled, the operating electric stress has increased more than eight times and the power loss factor in the insulation has been reduced very considerably.

It has already been mentioned that the first cables of importance were those used for submarine telegraphy in which a copper conductor was insulated with a coating of gutta-percha from the surrounding water, which acts as the return conductor. This form of cable remained practically unchanged until about 20 years ago. The demand for submarine telephony had become pressing and ways and

means were being sought whereby the electrical characteristics of the existing type of cable could be improved to accommodate the much increased frequency range involved. The problem of long-distance submarine telephony resolved itself into one of producing a cable with sufficiently low electrical losses. The most effective way of doing this was to improve the insulation, and not surprisingly the first step was concerned with refining the well-established gutta-percha.

Then in 1933 there occurred an event of outstanding importance to the submarine cable industry. During the course of researches into the behaviour of gases under pressures of the order of 1,000 atmospheres, Imperial Chemical Industries discovered that ethylene gave rise to a white wax-like solid which was quickly recognised as a polymer of ethylene. This material was found to have the right physical properties for submarine cable purposes and of no less importance it exhibited remarkably low electrical losses up to very high frequencies. However, its first important application was not to submarine cables but in cables to interconnect the transmitters and aerial systems of our early wartime radar installations. The exclusive availability of polyethylene (or Polythene) in this country played a vitally important part in helping us to gain the ascendancy in this aspect of our early wartime achievements. With the further development of radar technique which resulted from the introduction of the cavity magnetron operating at wavelengths of only a few centimetres much of the part which had been played by Polythene cables was taken over by other means of interconnection, but its submarine cable application became of increasing significance as the time for the invasion of the Continent approached. It had been realised from the beginning of the planning of this operation that immediate and reliable telephonic communication from headquarters to the bridgehead would be a vital necessity, and with this end in view stocks of cable insulated with Polythene were manufactured and stored at strategic points along the coast so as to be available at a moment's notice.

A very important post-war development —to which I will refer again later—has been the incorporation at intervals along the cable of submerged repeaters, their purpose being to provide periodic amplification of the telephone signals. Functionally these repeaters are similar to the valve amplifiers which form part of our radio receivers, but the conditions of operation are much more severe. Their component parts must be capable of long service under conditions which preclude normal servicing and their containers must be sealed at the cable entry and exit points by glands which will withstand the high external water pressure. The inclusion of repeaters at intervals of 40 to 50 miles along the cable length enables all the advantages of shortness to be obtained in cables of almost unlimited length. Several such cables insulated with Polythene have been laid during recent years, and it is to be hoped that the start of the projected transatlantic telephone cable, to be made of the same material, will not be long delayed.

My concentration on Polythene, and indeed on only a small region of its sphere of application, does scant justice to the immense progress which has been made during the past 20 years in the development, production and use of the class of synthetic materials known as plastics. But the range of types and the variety of applications of these materials is bewildering even to the well-informed, and I must pass to my next example.

I have already referred to the transformer as the device by which the voltage of an electrical supply is changed from one value to another. In its simplest form it comprises two coils of wire, the turns ratio of which corresponds to the voltage ratio desired, wound on a continuous core of magnetic material, and was invented in the early 1830's by Faraday as one of the means by which he demonstrated his discovery of the principle of electromagnetic induction. It remained unused and undeveloped, however, until the change from direct current to alternating current generation towards the end of the century, but it has since become a major item of equipment in our system of electrical supply, and single power-transformer units are now in operation rated at 190,000 kilovolt-amperes, and others have been constructed for voltages as high as 350,000 volts.

Such large transformers operate at full-load efficiencies as high as 99·4 per cent., but this figure by no means satisfies the transformer designer, since an increase in efficiency would not only result in a substantial saving of generated power in the country as a whole, but would also simplify the problem of cooling the individual units and therefore reduce their size and cost for a given power rating. The causes of this small, but expensive, inefficiency are the power losses in the resistance of the copper windings, about which the designer can do little, and in the magnetic material of the core. With the help of the steel manufacturers, he has been struggling to reduce these magnetic losses throughout the past 50 to 60 years. In the early days of power transformer manufacture the core material was mild steel assembled in the form of thin sheets,[1] or laminations, and a common experience was for the transformer efficiency to decrease progressively with time in service due to a progressive increase in the magnetic power loss in the steel—what was called magnetic ageing—until the resulting excessive temperature rise caused electrical breakdown of the windings.

This unhappy state of affairs led to an extensive search for an improved quality of sheet steel and a decisive advance occurred with the discovery by Robert Hadfield and his colleagues in Sheffield around 1900 that the addition of a few per cent. of silicon to steel of low carbon content reduced the magnetic losses by several hundred per cent., and also eliminated the troublesome phenomenon of magnetic ageing. Steady improvement has continued since this important discovery, notably by the almost complete elimination from the steel of small proportions of undesirable impurities such as sulphur, oxygen and phosphorus, as well as carbon, by high temperature annealing in hydrogen. In consequence, the last 10 to 15 years has seen a reduction in the magnetic power loss in British electrical sheet steel for comparable operating conditions by some 30 per cent.

During this period, however, a new possibility has emerged from scientific research on single crystals of ferromagnetic materials which is now becoming of considerable industrial importance in the above connection. The research showed that single crystals are highly directional in their magnetic properties. For example, the crystals of iron and silicon-iron are of cubic structure and it was found that they magnetise most readily in the direction of the cube edges and exhibit much superior magnetic properties in these, as compared with other directions of magnetisation. Now in transformer cores the magnetic field is directed along the plane of the sheets from which the core is constructed, that is in the direction in which the sheets have been rolled. It was too much to expect that the sheets could be made as single crystals—though this is not to be entirely excluded as a future possibility—but attempts were soon made to produce silicon-iron sheet material closely approaching the structure of a single crystal by arranging for the tiny constituent crystals to have their cube edges oriented in the rolling direction. This occurs to some extent in the normal hot-rolled sheet referred to above, but it was found possible to achieve a much increased preferred crystal orientation and much improved magnetic properties by following the hot-rolling process with an appropriate sequence of cold-rolling and annealing treatments. The new cold-rolled material is already being used extensively in the United States, and is now becoming available in considerable quantities in this country.

The improvement in magnetic properties attained in oriented sheet metal is attended by a number of applicational difficulties which are not yet fully overcome, but there is no doubt that the availability of this material will permit a further step forward in transformer technique.

In parallel with this progress on the power transformer side a rather more spectacular advance was occurring in the development of magnetic materials for telecommunication applications. Since the values of electric current arising in these applications are very small compared with those in power engineering the magnetic materials employed are usually operated at much smaller magnetising forces. The potentialities of the nickel-

[1] These sheets are insulated from each other electrically to reduce the eddy current component of the loss.

iron alloys in this respect began to emerge in the pioneer work of Hadfield which led him to the development of the silicon-iron alloys mentioned above. By the early 1920's the use of nickel-iron alloys was well established in telecommunication equipment, and later work took the form of studying the effects of adding further elements such as molybdenum, chromium and copper until in the mid-1940's there was produced in America an alloy of iron, nickel, molybdenum and manganese, known as Supermalloy, of quite exceptional properties. These various alloys were produced in sheet or strip form, and used in much the same manner as in power transformer construction, though the overall sizes bore no comparison. However, an adequate reduction of the eddy-current component of magnetic loss at frequencies of several million cycles per second demands a much finer sub-division of the core material than is possible with the laminated construction. The requirement was met by preparing the magnetic alloys in powder form, coating the individual particles with an insulating film and compressing the powder to the desired core shape. Even this advanced, powder core, technique is now being superseded through utilisation at long last of the well-known magnetic properties of the iron oxide, Fe_3O_4. During the recent war the effect of replacing one of the iron atoms by the atoms of other elements such as nickel, zinc, manganese, etc. was studied extensively in Holland, and there resulted a very important new class of magnetic materials, known as the Ferrites. These materials have an extremely high electrical resistivity compared with metals so that it is not necessary to grind them to a powder and insulate them as in the powder core technique in order to reduce eddy-current loss. They show promise of finding extensive application in the further development of telecommunications.

I come next to a quite different way in which progress can occur—by the discovery, and recognition of the potentialities, of some special property of a material leading to the evolution of a new engineering technique.

A good example of this is afforded by the post-war discovery and development of the germanium crystal triode, known as the Transistor; an amplifying device capable of performing most of the functions of the thermionic triode valve but much simpler in construction and much smaller in size and having the great advantage for many purposes of not requiring a heated cathode as its source of electric charge carriers.

To introduce this discovery I must take your mind back to the early days of wireless telephony, when the detector employed consisted of a small crystal of natural silicon and a contact wire, or cat's-whisker, of tungsten. Some of you will remember that the chief characteristic of these early crystals was the need for incessant probing with the cat's-whisker to find a sensitive point on the crystal surface and the ease with which the point could be lost on the least vibration. With the advent of the stable thermionic valve, the crystal fell into disuse, to the regret, I think, of many, for with it went much of the early fun of wireless.

When, however, during the early 1930's research work was started on the generation of oscillations of centimetre wavelengths, with corresponding frequencies in the hundreds of megacycles, the lack of a thermionic valve capable of operating as an efficient detector at these frequencies caused a return to the crystal detector. In 1940 the invention of the cavity magnetron made large centimetre-wave powers available and the crystal was introduced into service radar equipment as a matter of necessity notwithstanding the grave dangers associated with its lack of stability. It was regarded, of course, as no more than a stop-gap which would shortly be superseded by a thermionic valve, but in fact no such suitable valve became available and manufacturing methods were developed during the early war years which made the silicon crystal detector as stable and rugged as a valve.

During the course of this early development work, the study of other materials established that silicon was in fact the best for use at the highest frequencies available : but it was found to be markedly inferior to germanium, an extremely rare metal in purified form, in its ability to withstand voltage in the high resistance direction of operation and was therefore

relatively easily damaged by strong signals —the process known as burn-out.

In consequence although germanium did not replace silicon in centimetre-wave detectors its ability to withstand high reverse voltages began to stimulate a demand for germanium rectifiers for low frequency applications as an alternative to the thermionic valve, copper oxide and selenium rectifiers in widespread use.

The first problem was of course to produce this scarce and inaccessible metal in adequate quantities. It was found to be present in concentrations around 0·01 per cent. in some of the ores from which zinc and cadmium are extracted and also in small proportions in the flue dust from certain classes of British coal. The second problem arose from the

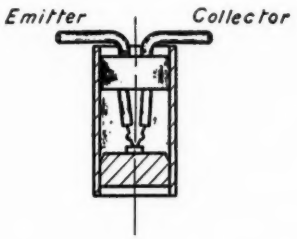


FIG. 1(a).

POINT-CONTACT TYPE TRANSISTOR.

observation that the merits of germanium as a rectifier material were markedly dependent on its purity and that the presence of certain kinds of impurity atoms in concentrations of less than one part per million seriously affected its performance.

The solution of these extraction and purification problems has involved the expenditure of very extensive effort and called for great metallurgical skill and ingenuity.

However, important though it is in itself, the development of the germanium diode proved to be only the forerunner to the revolutionary discovery in 1948 of the germanium triode. The construction of this device in its original point-contact form is illustrated in Fig. 1 (a), and a simple equivalent electric circuit is drawn in Fig. 1 (b).

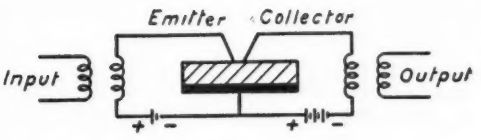


FIG. 1 (b)

SIMPLE EQUIVALENT CIRCUIT OF POINT CONTACT TRANSISTOR.

It has three electrodes of which the so-called emitter and collector electrodes consist of cat's-whisker contacts located a few thousandths of an inch apart on one face of the germanium crystal, and the third, the base, electrode covers the other face. Although close comparison with the thermionic triode is not altogether profitable, these electrodes may nevertheless be thought of as corresponding to its cathode, anode and grid electrodes respectively. It is not possible here to explain the mechanisms within the germanium which govern the operation of the device, but what happens externally is that a variation of electric current in the emitter circuit due to an applied signal voltage produces a sympathetic current variation in the collector circuit from which considerable voltage and power amplification of the signal can be obtained. In some respects this form of transistor is still in the development stage but it is now in commercial production and is beginning to find many varied applications. And yet, as evidence of the rapidity of present-day scientific and technological progress, it is already in process of being superseded by the junction-type transistor illustrated in Fig. 1 (c), which has the important advantages of being less 'noisy,' of permitting amplification over a wider range of frequency in the input signal and,

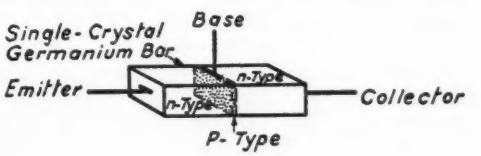


FIG 1 (c).

THE n-p-n JUNCTION TRANSISTOR

because of the greater surface area of its electrodes, of handling much increased signal power.

A few remarks are necessary about the regions of the germanium marked *n* and *p* in Fig. 1 (c). Those marked *n* contain small proportions, of the order of only a few parts in one hundred million, of impurity atoms such as antimony or arsenic which endow the germanium crystal lattice with one excess electron per added impurity atom. The excess electrons may be regarded as 'free' to move about within the lattice, and the electrical conduction in the *n* regions takes place by means of these electrons. The *p* region on the other hand contains a similarly small proportion of atoms such as indium or gallium which produce an electron deficiency of one electron per impurity atom—so-called positive holes—and current flow in it is known as positive hole conduction.

The production of these junctions starts with the preparation by successive recrystallisation processes of highly purified single crystals of germanium which are cut into thin wafers and these into tiny pellets. For *n-p-n* junctions the pellets must initially be of *p*-type germanium throughout and the *n* regions are then prepared by diffusing into them by thermal processes the appropriate kind and concentration of impurity atoms.

The volume of experimental forms of *n-p-n* transistors is of the order of only $\frac{1}{500}$ cubic inch as compared with $\frac{1}{8}$ cubic inch for present-day sub-miniature thermionic valves and 1 cubic inch for miniature valves, and the device has the added merit, already mentioned, over the valve that it does not incorporate a heated cathode and therefore avoids the electrical supply needed by the valve for this purpose. These are great advantages for such purposes as submarine cable repeaters and guided missiles where the reduction of the size of components and of the operating electrical supplies to a minimum are of vital importance, and also in the construction of complex equipments such as electronic computers incorporating in their existing form many thousands of valves. Much remains to be done before the junction transistor becomes as reproducible in characteristics and as versatile as the valve, but a very important future lies ahead for it. There is no reason to doubt that more intricate devices analogous to multi-electrode thermionic valves will be developed in due course, and that the power which they are capable of handling will be increased considerably from its present level. This is evident from the rapid progress being made in the development of *n-p* junction diodes for power rectification purposes.

As a second illustration of the emergence of a new engineering technique from the discovery of a special property of a material, I move to a quite different field, that of the conversion of the nuclear energy of uranium—an element of almost insignificant engineering importance less than fifteen years ago—into electrical energy, and I will try to indicate how the practicability of this conversion is dependent on the use of other materials also possessing special properties, though of a different kind.

This new branch of engineering has arisen from the discovery that the nucleus of the uranium isotope of atomic weight 235, written U_{235}, can be split by the entry into it of an uncharged neutron of suitable energy, and that in the disintegration other neutrons are made available to continue the process. The aggregate mass of the fission products is less than that of the original U_{235} atom and this mass difference appears as free kinetic energy of total magnitude around 200 million electron volts per fission. The average number of neutrons produced per fission is $2{\cdot}5 \pm 0{\cdot}1$, and their median energy is about one million electron volts.

For the purpose of an atomic bomb the ideal is for all the fission neutrons to produce further fission in a chain reaction, but in a nuclear reactor to be used, in part at any rate, for external power generation over long periods of time, the fission rate must not be cumulative but be kept substantially constant. The reactor will continue in operation so long as one of the neutrons ejected per fission produces further fission ; that is, if, on the average, not more than 1·5 are used up in other ways.

Natural uranium contains only 0·7 per cent. of fissionable U_{235}, the remaining 99·3 per cent. being the isotope U_{238},

and with this material as fuel considerable ingenuity is required to establish and maintain the reaction. Thus some of the ejected neutrons come into collision with and are absorbed by the surrounding U_{238} atoms, with the resulting production of the important element plutonium, while others are absorbed unprofitably in the materials constituting the structure of the reactor. It is essential for one neutron per fission to survive absorption in these ways if the fission process is to continue.

Now the statistical probability of the fission capture of neutrons by U_{235} is

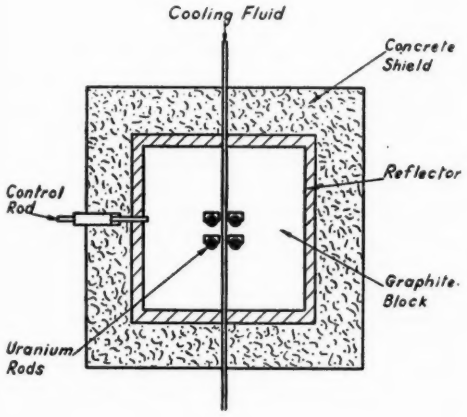


FIG. 2.

SCHEMATIC REPRESENTATION OF A NUCLEAR REACTOR.

greatest when the neutrons are of quite low energy, around 0·03 electron volt, so that the fission neutrons must be slowed down to this energy value from the million or so electron volts which they possess initially. To do this the uranium fuel is arranged in the form of an array of parallel rods and is surrounded by a medium known as a Moderator in the manner illustrated in Fig. 2. The neutrons undergo successive collisions with the atoms of this medium, and lose energy in the process, before diffusing back into the fuel elements at energies at which the probability of fission capture by the scarce U_{235} is high.

The essential requirements of the moderator material are that it should slow down the fission neutrons to the desired energy in a minimum number of collisions and with minimum loss of neutrons in doing so, and these demand that it shall be composed of atoms of low atomic weight and low neutron absorption characteristics. The choice so far has been between three substances—highly purified graphite; light water, H_2O ; or heavy water D_2O, deuterium being the heavy form of hydrogen.

Of these, heavy water is better in both respects than graphite, and to such an extent as to reduce by a factor of ten the minimum size of operable reactor. On the other hand heavy water does not provide the structural support for the fuel elements afforded by graphite blocks and is much more expensive. It is not many years since this substance was a scientific luxury made with difficulty and in very small quantities, and even now after extensive large-scale research and development work the cost of its production by successive electrolysis of ordinary water is around £75,000 per ton. During the last few years a search has been made for a more economic means of producing it and a combined New Zealand-United Kingdom team has recently evolved a method of utilising the steam which pours from the ground in the volcanic regions of New Zealand.

Light water is of course vastly more economical than D_2O in itself, and being lighter slows down neutrons more effectively : but its neutron absorption, and therefore the neutron loss, is several times higher. This demands the use of enriched uranium fuel, that is fuel in which the U_{235} proportion is above the figure for natural uranium, and the cost of producing this enriched material is at present so high as to leave uncertain whether the use of light as against heavy water would in fact be economically advantageous.

The energy released in the fission process expressed in electrical terms amounts to some 22,000 units of electrical energy (kilowatt hours) per gram of U_{235}, and the purpose of the reactor cooling system shown in Fig. 2 is to remove as heat such of this energy as is not absorbed in other ways and at a temperature which is safe for the reactor and also serviceable for power generation purposes. Five substances have been used or are under

consideration as the cooling medium—two of them, helium and carbon dioxide, are gases, and the others are liquids, namely light water, heavy water and liquid sodium or sodium-potassium alloys. The power reactor now under construction in Cumberland uses graphite as moderator and carbon dioxide at several atmospheres pressure as cooling medium. The extensive use of liquid sodium or one of its alloys no doubt lies many years ahead but it has several advantages over the other possibilities, notably that of permitting a much higher operating temperature and therefore an improved thermal cycle in the power generator unit. I do not find it easy, remembering my schoolboy experiments with small pieces of sodium, to think of it as an important engineering material of the future, but it seems certain that this will prove to be the case.

But perhaps the most important materials problem in reactor design is that of encasing the fuel elements so as to prevent contact between the uranium and the cooling medium. The metallurgical requirements for the sheathing material are extremely severe and restrictive. Thus, it must be capable of fabrication in the form of thin-walled tubes ; have adequate strength at the elevated temperature of the fuel elements to withstand the thermal and mechanical stresses imposed on it ; its mechanical properties must not deteriorate under the intense neutron bombardment to which it will be subjected ; it must not corrode as the result of contact on one side with the fuel elements and on the other with the cooling medium ; and it must exhibit low neutron absorption. As already mentioned the latter imposes the necessity for a metal of low atomic weight, and the designer finds his choice limited to such metals as aluminium, magnesium, beryllium and zirconium. Aluminium has been used in low temperature reactors for research purposes, but, as with magnesium, its melting point is too low and its mechanical properties inadequate for future power reactors. Considerable attention is therefore being given to the production of beryllium and zirconium, and it is again illustrative of the pace of scientific and technological advance that whereas the latter metal hardly existed some five years ago it is now being produced in the United States in highly purified crystal form at the rate of tons per month.

This review of the impact of improved and new materials on engineering development has necessarily been highly selective and very incomplete. I might have dealt, for example, with equal justification, with the many aspects of the subject of powder metallurgy, with the new ceramic materials based on barium titanate, with the rapid emergence of titanium as a structural material, or with the beneficial changes in the physical properties of certain plastics which result from their irradiation with high energy particles. But I hope I have said sufficient to illustrate the intimate inter-relationships and the growing interdependence of progress in chemistry, physics, metallurgy and engineering. A step forward in scientific knowledge and understanding stimulates the development of new techniques and these in turn react back on the science from which they spring and make possible still further scientific advance. The science of today is the technology of tomorrow ; the time scale is shrinking rapidly ; and it becomes increasingly difficult for the layman to gain even a superficial appreciation of what is happening and what it all portends. Yet it is of the greatest importance that he should, for he is subject to its influences and its consequences at every turn. In seeking to present and interpret the advance of science to the general public the British Association has a great mission to fulfil, and I hope I may have made some little contribution to its fulfilment.

COLONIAL ARCHAEOLOGY

Address by

SIR MORTIMER WHEELER, C.I.E.

PRESIDENT OF SECTION H

THE British Association is traditionally a platform, less for the promulgation of new knowledge, than for the adjustment of knowledge, new and old, to the miscellaneous society in which we live. In line with that tradition, I present to you to-day, not a new piece of scientific research, but a social and administrative problem of high importance to the disciplines which we serve, and of high importance, incidentally, to the prestige of the Commonwealth which our Association represents.

Let me say at once what my theme is to be. It is the need, the very urgent need, for the safeguarding of cultural and, in particular, historical and archaeological evidence in and relating to our Colonial territories. Under the term 'safeguarding' I include the integral task of rendering that evidence reasonably accessible to study.

In discussing this pressing problem I shall have some hard things to say. These hard things must be said if the nature of the problem is to be appreciated. But I would make it clear at the outset that, in saying them, I have no wish merely to stir up mud. I have no wish to attaint any member of our present Colonial Office or of our present Colonial administrations. Our present Secretary of State for the Colonies, Mr. Lyttelton, is perhaps the most harassed and distracted man on earth; nevertheless I have reason to suppose that he is neither unaware of, nor unsympathetic to, what I shall have to say. His colleagues in the Colonial Office are certainly apprised of the problem. Whether its full significance is adequately appreciated in all quarters remains to be seen. Meanwhile, let me make it clear that my concern is, not to impugn individuals, but to affirm a need and in some small way to prepare a climate in which it can be effectively considered.

As a nation we are, I suppose, as baffling a mixture of opposites as one could find in a day's flight: at least, so our French friends are never tired of telling me. Our strong suits, they slyly admit, are Justice and Sanitation; but when it comes to things of the mind, to cultural matters, we have to confess that we are liable to be curiously myopic, if not actively resistant. In India we were saved at the last minute from wrecking, or acquiescing in the wreckage of, an irreplaceable cultural inheritance by the action of *one* man. That man was, of course, Lord Curzon; without Curzon, our cultural record in India would be as black as our enemies like to pretend that it was. In Africa we have had no Curzon; and it is from British East Africa that my cautionary tales will mainly be derived to-day by way of preface. I cannot pretend to think that others of our Colonial territories could not produce similar melancholy stories.

My information, which comes from unimpeachable first-hand sources, relates mainly to archaeological material, but in citing examples from it I am not blind to the collateral question of written records, and some slight reference to these will be included. The principal area covered by my information includes Zanzibar, Kenya, Uganda and Tanganyika: in other words British East Africa. But the problem spills over into the Somaliland Protectorate, and indeed extends to the mouth of the Red Sea and across to Aden. For two thousand years or more the coastal tracts of this vast region have been in touch with outside forces coming from the Mediterranean, Arabia, Persia, India and China, and these forces have affected native conditions and cultures to an extent which is at

present largely a matter of conjecture but was certainly appreciable. In the interior, our information is even more defective, and much work will have to be done in the equation of native cultures with dated intrusive evidence before any methodical classification of them can be attempted. South India presents a parallel problem ; there in recent years the careful relation of imported material along the coast with native material extending far into the hinterland has produced results which amply indicate the possibilities of research elsewhere along these lines. I mention this analogy merely to emphasise the desirability of opening up East Africa to systematic research and, meanwhile, the pressing need to catalogue and preserve the material that has long awaited attention there.

What is in fact happening to this cosmopolitan and African heritage? My examples—and they are only examples—will tell you. They relate wholly or mostly to the last ten years and cannot therefore be dismissed lightly as the sins of a remote and archaic age.

I quote first from Tanganyika. There, particularly in the south, destruction has been rife. Perhaps the most beautiful monument along the coast was the palace mosque of Kilwa island. I say 'was' because it owed not a little of its beauty to the Persian tiles, probably of the Kashan period, with which it was covered. An individual bearing a familiar name, apparently then in government service, decided that these tiles would make admirable souvenirs. He employed workmen to remove them ; in the course of the work two of the pillars were knocked down and a dome collapsed, but the tiles were safely removed. A little later, the tile-robber retired to Cape Province, taking his ill-gotten booty with him ; but the tiles were broken in transit and were subsequently all thrown away. Nor was this vandal alone in crime. The then District Commissioner is stated also to have removed Persian tiles from this or another building. To-day there are no tiles on the island.

This process of depredation is, of course, no new thing. On the neighbouring island of Songo Mnara the palace mosque was long ago described by Sir Richard Burton as covered with blue tiles. They are no longer there. On the same island the palace of the Sultans of Kilwa has a vaulted roof with a hundred cavities on the interior for ornamental bowls. Only six of these are left *in situ*, but it is said that others were not long ago in the possession of a local District Commissioner. It is fair to infer that they, too, are 'on their way out.'

But straightforward robbery was not the only archaeological crime on Kilwa and Songo. Recently in the name of science excavations have been carried out there by an individual, who shall be nameless, but whose standards are sufficiently suggested by his statement to the American press that he was excavating a palace of the Queen of Sheba that resembled Versailles. In fairness to the local authorities I should add that this excavator would probably not have received a permit to dig had he not arrived with a letter from the Colonial Office.

The tale of destruction could be continued. Further up the coast, at Bagamoyo, carved woodwork, including ornamented doors, was removed and sold in the recent 'forties without a thought for its local historical importance. At Sadani and Moweni, sites formerly protected by thick bush have been cleared by the District Commissioner ; sixteenth-century carvings have been destroyed, and tombs are being broken up for lime. A similar fate has (since 1945) attended pillar-tombs and part of a ruined fifteenth-century mosque at Kunduchi. At Msasani celadon and blue-and-white insets in a series of tombs have been removed as curios within the past few years. At Kaule recently all the Chinese porcelain has been removed from the pillar-tombs there.

For Zanzibar I will quote *verbatim*. A correspondent states : 'When I visited Zanzibar in 1947, the chief monuments of the Omani Sultans were the great palaces of Chukwani, Beit el Ras, and Mahahubi. They were unique in representing the strange amalgam of culture in the Empire of Zanzibar between 1780 and 1860, Persian, Arab, Indian and Portuguese. On my last visit in 1951 I found that Chukwani had been completely demolished—I was told, on the authority of the then Colonial

Secretary—and the Beit el Ras had been chosen as a site by the Education Department who were erecting a modern school in its place. One great horse-shoe arch with a faïence inlay survived at Beit el Ras. I pleaded with the British Resident that it should be preserved. He was most sympathetic, but I was later informed that the Education Authorities had decided otherwise, and that it, too, had been demolished. Mahahubi has simply been allowed to fall into decay like the medieval ruins on Pemba and Tumbatu islands.'

In Kenya the rate of destruction in the coastal area appears to be even higher than in Tanganyika, and I am sorry to learn that the major criminal is there the Public Works Department. The great fortification of Siu has completely vanished, its materials used for the embankments on the Lamu-Malindi road. And whether at the hands of the Public Works Department or at those of uncontrolled Indian contractors in search of cut stone, a part of the ancient walled towns of Mtwapa and Old Kilifi, with their medieval mosques and tombs, has gone the same way.

Nor is the P.W.D. of Uganda exempt from blame, for all the enlightened sympathy of the present governor. In 1950 it drove a road straight through three large and ancient mounds at Ntusi, and levelled the summit of a tumulus, the traditional grave of the Bachwezi, to build a house for a local native official. In the latter case a number of the grave-goods were left exposed on the surface, and were salved by Mr. Eric Lanning while on a visit.

These various examples—and I would repeat that they are merely examples—combine to indicate a widespread indifference, ignorance or impotence. Most of them represent harm done by government itself, largely through its Public Works Department. But the damage has not been confined to actual destruction in the field; of equal concern to right-minded people is the absence of adequate provision for the preservation of movable antiquities and records and of the information relating to them. The deficiency of equipped museums in East Africa is itself an index of our obliquity in cultural matters there. In Tanganyika there is indeed a museum of a sort at Dar-es-Salaam, but it is in great need of expansion, its registers pitifully imperfect, and it seemingly has no working policy. In Zanzibar is a small museum for which a curator was recently advertised, but the Secretariat archives, which are said to be very important, are without an archivist. Steps are, I understand, being taken to develop an archaeological museum in Uganda, but that at Mombasa for Kenya exists only in plan, and that of Hargeisa in British Somaliland is no more than a pious hope. In Uganda, where the Governor has, as I say, an exceptional interest in such matters, the Secretariat archives for 1800–1900 are missing, whilst for the remainder there is a rather damp, unsatisfactory depository at Entebbe, but no archivist. Kenya is likewise without an archivist, though the district records need protection and the Secretariat papers, destroyed in 1939, need replacement from London. In summary, the general picture as regards records in East Africa is one of sporadic activity, but nowhere is there a qualified archivist either on the job or in training.

Then, peripheral to my main theme but in itself of high importance, is the absence of any recognised and regular media for the publication of our African material. At the lowest, the absence of such media is itself a stimulus to indifference. On a higher level it is unnecessary to emphasise before Section H the constant frustration experienced by students and scholars in the lack of a focus for research and information. On the present occasion I will say no more about this vital matter, but my task would be very incomplete without a passing reference to it.

Now, still retaining East Africa as our test-area, let us glance at the constructive side of our problem. What has in fact been done to conserve this cultural heritage of which I have been speaking? First, we shall be told that each of the territories now in question has an Antiquities Law. In a very limited sense, that is true. But let us glance at these laws, and then let us review the existing machinery for carrying them into effect.

The law for Tanganyika, dated 1947, is little more than a page—to be exact, 368 words—in length. Its initial definitions are vague and incomplete; it contains no general regulations covering the

discovery or export of movable antiquities ; there is no clear definition of the conditions under which excavation may be carried out ; there is no provision for the maintenance of ' monuments ' ; no procedure is laid down even for scheduling sites, or for the promulgation of such scheduling. The Governor ' may by order declare any monument or group of monuments to be a protected monument,' but there is no hint as to his sources of information or advice. In short, the puny Act may be dismissed as little more than eye-wash.

The Ancient Monuments or Antiquities Acts for Zanzibar and the Somaliland Protectorate are a little more substantial, but that for Uganda is as bare and boneless as that of Tanganyika and strangely omits to control any damage whatsoever other than that caused by illicit archaeological excavation. In Kenya there is an ' Ordinance for the Preservation of Objects of Archaeological and Palaeontological Interest (1934),' but the procedure for getting a monument scheduled is so cumbrous as to be practically unworkable, even were there an informed staff to work it. It is supplemented feebly by a passing mention in the Act governing the National Parks, wherein subsections (h) and (i) to Section 12 make it unlawful ' to remove from a national park any object of geological, prehistoric, archaeological, historical or other scientific interest ' ; or ' destroy or deface any object whether animate or inanimate in a national park.' Otherwise the cultural inheritance contained in these reserves is nowhere mentioned.

The pitiful inadequacy of all these laws is the more surprising when it be recalled that, just across the water in Aden, is a sound and sensible Antiquities Ordinance which provides an admirable model for enactments of the kind. The antiquities of the colony of Aden were at one time administered from India, and the Aden law was evidently based on the Indian law, which was drafted with very considerable care. Having said that I must add parenthetically that the model law of Aden is in effect a dead letter. It is significant that a nominal part-time director of archaeology there receives the munificent salary of £100 a year for his services, and about £35 are allocated to him for the purchase of antiquities. If, however, I embark upon our sins of archaeological neglect in Arabia I shall never end ; in the present context it will suffice to commend the Aden Antiquities Law as a formal model, without in any fashion commending the measure of its implementation. But even were the Aden example not available, there are well-tried ordinances in Iraq, in the former Palestine, and in Cyprus, which might profitably have been consulted by the law-givers of East Africa. It would have been no hard task to adapt these laws to local conditions whilst retaining something of their comprehensiveness. And, incidentally, why this chaotic diversity in the laws of adjacent territories faced by closely comparable problems ? Surely a uniform model for the whole of East Africa, enacted territorially with such minor modifications (if any) as might be locally desirable, would be the common-sense solution of this universal problem ?

But behind all this business of form and enactment lies a factor of fundamental import. Laws are worse than useless without the machinery for their enforcement. And not in one of the territories in question is there a significant vestige of machinery for the proper enactment of an antiquities law. In some of them it is the Governor, with or without the aid of his District Commissioner, who from his inner consciousness determines what is or is not an antiquity worthy of preservation. In another, it is the British Resident who is responsible. In yet another, it is a board known as the National Parks Trustees who, amongst many other duties, classify objects of archaeological and historical interest ; and listen to its composition ! It is composed of :—

The Chief Native Commissioner.
The General Manager of Railways and Harbours.
The Conservator of Forests.
The Game Warden.
Three persons appointed by the Governor.
Three persons nominated by the Legislative Council.
Two persons appointed by the Association of Chambers of Commerce.
One person nominated by the Board of Museums' Trustees ;

and one person appointed by the East African Professional Hunters' Association.

Admittedly this body is only incidentally concerned with antiquities. But that is precisely the trouble. Its composition sufficiently indicates the exceedingly low priority given to cultural matters in the territory concerned.

And yet, paradoxically, it is precisely in this territory, Kenya, that most has been done to secure an understanding of the value of these things. The principal archaeological sites on the coast of Kenya have been listed, several of them have been surveyed, and some have been excavated. The Oxford University Press has just published a monograph describing one of these excavations in workman-like fashion. But do not let this golden exception bedazzle us. It is due, not to the wisdom and virtue of high-level government, but to the personal initiative of a particular National Parks Warden who has other duties to perform. It is an accident, none the less pleasing for that, but not a matter for excessive congratulation. If the Warden in question were to depart, the work would almost entirely cease. There is no long-term policy, no government guarantee, behind it. What it *does* do is to show how productive such policy and guarantee could be with relatively small financial outlay.

And here I must interpolate another enterprising and singularly productive intervention of a largely non-official nature. During a series of years the Reverend Gervase Mathew has paid intermittent visits to East Africa, and his investigations further emphasise how much can be done with how little provision. During 1947 he carried out an archaeological reconnaissance on the coast of Italian Somalia, and later in the same year on the coast of Portuguese East Africa. Between these flanking territories he undertook in 1950–52, in liaison with the governments concerned, preliminary archaeological surveys in Tanganyika, Uganda, and the British Somaliland Protectorate. On the basis of these surveys he has been able to classify the coastal remains, from the Ptolemaic period onwards, tentatively into four main phases, with cultural subdivisions, and to indicate key-sites in connection with them. He has rightly emphasised the desirability of working into the interior from the coastlands, where contact with the Mediterranean, India, China, and perhaps Persia, should give fixed points and facilitate the classification of home-grown industries and cultures. In other words, the relatively brief visits of an interested visitor have begun to lend a vertebrate character to the hitherto inchoate mass of East African material which, save in the processes of destruction, has been almost untouched by official hand or mind.

Gervase Mathew's pioneer classifications bring me back to the fundamental question of conservation. Even if each territory possessed an efficient antiquities law and an adequate executant staff, that staff could not be expected to operate the Act efficiently without careful overall planning : that is to say, without a comprehensive understanding of the historical and archaeological issues involved. In no country, with the best will in the world, can anything approaching total preservation be achieved. The question of priorities arises from the outset, and it stands to reason that, without a scale of values, priorities cannot be established. Discriminating work such as that initiated by Gervase Mathew and, in Kenya, by Mr. James Kirkman must go hand in hand with, be integral with, the task of preservation and control.

Now let me summarize what I have said up to the present stage. First, I have stressed the deplorable destruction of archaeological, architectural and artistic material in our East African possessions during recent years down to the present day. The rate of destruction has greatly increased since the war, primarily owing to the increasing activities of the Public Works Department and their avidity for readily accessible cut stone. These and other official departments and individuals are not always blameworthy ; they have never been taught to respect the monuments of the past, and there is no one to teach them. That brings me to the second point, of which I have already spoken. In none of these territories is there an Antiquities Act up to modern standard—up to the standard which has obtained in India,

Iraq, Palestine, Cyprus, and the United Kingdom for more than a quarter of a century. Thirdly, there is no trained staff or establishment, however small, to implement intelligently such ordinances as there be ; thus, for example, there is nowhere any official schedule of ancient monuments in East Africa. And fourthly—arising out of these three basic defects—no background of informed and cultivated public opinion has been created in these territories ; and as a crowning factor, with the devolution of political power into native hands, the creation of such opinion becomes simultaneously more difficult and more urgent.

From this dark picture of British East Africa I turn to the somewhat brighter vista of British West Africa. Not that West Africa offers abundant cause for congratulation ; there is still nearly everything to do there ; but certain initial steps have at least been taken there on the educational side. In particular, the University College of the Gold Coast, at Accra, now has in A. W. Lawrence an experienced professor of archaeology, with just that Mediterranean training which is most likely to offer some of the required basic perspective ; and in Dr. Fagg the College possesses a historian who has for many years worked fruitfully in the African field. In the Gold Coast, too, some steps have been taken to conserve government archives, whilst in Nigeria the central archives are in good order. All this is on the plus side. For the Gold Coast, at any rate, we may now expect a phase of planned exploration. For the rest, it is fair to say that no properly established department of the kind exists in any of these territories. Out-of-date and unsatisfactory ordinances are extant, but in the absence of suitable executive staff there is no pressure of opinion either within or without government circles which could render them effective, such as they be. I shall be told that Sierra Leone has a sort of Monuments Law and even a ' Monuments and Relics Commission ' ; but it has no department and no museum. I shall be told, too, that Nigeria has a formal Antiquities Branch ; but listen to a few sentences from a relevant annual report laid on the table of the Nigerian Legislative Council : 'At the beginning of the year,' we are informed, ' the Archaeologist at Jos had insufficient storage in an isolated corn store in open farmland and in a small office building on the fringe of the town. He rented twelve miles away near Bukuru a house which was fortunately large enough to give him room for a dark-room and workshop. This house has now been pulled down for mining by the company which owned it. The Archaeologist after returning from leave was given a temporary house at Jos but there is no room for a workshop.' Or again : ' In 1947 it had been proposed that the Headquarters of the Surveyor of Antiquities and the storage for the Central Museum should be made temporarily at Owo in Ondo Province until a satisfactory site could be found in Lagos. . . . Early in the New Year, however, the Finance Committee rejected the proposal when it was asked to provide the £8,000 estimated to be the cost. . . .' Subsequently a plan was proposed for the necessary accommodation at Lagos itself, but this was likewise turned down and, adds the report, ' there is little prospect of getting any building in the near future. Throughout the year the Surveyor has had no proper office and has therefore had to use a room in his house.' Later, three small rooms were allotted to the Antiquities Branch by the Education Department, but, we are informed, ' these rooms are so congested that there was difficulty in getting space to unpack the boxes. . . . One store now contains ivories, metalwork, and chemical stores . . . one office contains the collection of wooden articles which are piled up in cupboards and on the floor, and the other office is occupied by the carpenter and caretaker, and by timber and miscellaneous stores.'

In all this one cannot help recognising something of the aura of the old Slave Coast, in which human beings and human interests were mere expendable stores. It is not surprising in the circumstances to hear from Professor Lawrence that ' over the entire region of British West Africa barely a dozen small attempts at excavation have yet been made ' ; and as an instance of the barrenness of our knowledge he observes that virtually nothing would be known of Nigerian prehistory but for accidental discoveries of palaeo-

lithic implements in the open-cast tin-mines of central Nigeria.

By and large, this is a sad, deplorable tale. The one hope at present rests with the university colleges, which are working on the right lines; but their history departments are very young and find it difficult to get good staffs. Much that I have said about the needs of East Africa applies to the West. The one slightly redeeming feature in the West is, as I say, the presence there of a somewhat more advanced educational environment.

For comparison, let us turn aside for a brief moment to recall what the French have done. The Institut Français d'Afrique Noire (IFAN) at Dakar has organised services (1) for the collection of archives, both centrally and in the districts; (2) for the preservation and exploration of sites of historical importance; and (3) for the quick publication, on some considerable scale, of results on both the archaeological and archival sides. The publications consist of well-produced monographs, government reports which are often contributions to learning, and fairly simple pamphlets about work done or in prospect, especially by amateurs who are attracted to the field. As usual, the French are far ahead of us in organisation and production. In saying that I have no doubt that here, as elsewhere, we could, if we tried, equal or surpass them. But at government level, abroad as in our own country, we have been unconscionably slow starters: and the remaining time, let me emphasise, is short.

For completeness, I mention in passing two bodies which are peripheral to the matters now under consideration. The first is the International African Institute, of which a past-president of Section H, Professor Daryll Forde, is director. This Institute is, I understand, mainly concerned with anthropological and linguistic work. It is a going concern but lacks adequate funds. The other body is the Scientific Council for Africa south of the Sahara (the so-called C.S.A.). This was set up at Nairobi in 1950 by agreement between the Governments of Belgium, France, Portugal, the United Kingdom, the Union of South Africa and Southern Rhodesia, and its members are appointed as representative of a variety of scientific subjects amongst which archaeology is not at present included. This council may encourage a certain solidarity in African Research and help a little to focus opinion, but it is not, and does not pretend to be, any sort of palliative to the ills of which I have been speaking. To those ills I now return.

In July 1953, a widely attended Conference on African History was held in London under the chairmanship of Professor C. H. Philips, of the School of Oriental and African Studies. The whole field was surveyed by delegates from the various British and French territories concerned and by others working in that field. At the end of the Conference a series of Resolutions was carefully drafted to embody certain clear decisions which had emerged from the three-day discussion, and I cannot do better than give these Resolutions such additional publicity as this platform proffers. There are six of them, as follows :—

(1) The Conference was impressed by evidence that the outlines of African history, which is of such urgent importance for African education, could be reconstructed by the co-operation of historians and archaeologists.

(2) This would imply the adequate preservation of ancient and historical sites, monuments and documents, and the development of museums and public record offices.

(3) The Conference was concerned at evidence that despite the progress made in recent years by several governments, the rate of destruction seems to be steadily increasing, while insufficient steps are being taken to arrest deterioration. The Conference felt that there was need for the expert and effective revision of existing Ordinances for the preservation of sites and other Monuments, and that the safeguards should include provision for the scheduling and preservation of Monuments, and for the control both of excavation and of the export of antiquities.

(4) Ultimately, the development of African archaeology depends upon the formation of adequately staffed departments of antiquities in the territories concerned. Only this can create the conditions necessary for the adequate conservation of sites and antiquities, for planned fieldwork and the steady publication of results.

(5) Of equal urgency is the creation or improvement of record offices, under qualified archivists, whose duties should include the supervision of provincial and district records, the assembling of other documents and the provision of facilities for study.

Until this is done no definitive history of the European connection with Africa or of the African peoples can be attempted.

(6) As an immediate step, it is desirable to establish a School of History and Archaeology in East Africa organised on the analogy of the British Schools at Rome, Athens and Ankara, which could act as a field training school for students.

It is unnecessary at this stage for me to amplify those six Resolutions. They are the corollary of what I have already said. The vital remaining question is one of ways and means, and I can at the present moment carry the matter only a short step further.

The Resolutions were transmitted to the British Academy which, after careful independent investigation, passed them on to the Colonial Office. There they were sympathetically received, and discussion ensued between the Secretary of State for the Colonies and representatives of the Academy. In consequence, the Academy has now, in June of this year, armed itself with that inevitable instrument of British democracy, a Committee, set up with the expressed approval and co-operation of the Colonial Secretary for the purpose of considering and advising upon cultural matters relating to all territories administered by the Colonial Office. That Committee has already got to work, and at the present stage we can only wish it the success which the problem urgently demands.

So far I have limited my remarks mainly to East and West Africa, and it is to those regions that my enquiries have been primarily directed. But in varying degree I have no doubt at all that the problem is one of almost universal application within our Colonial territories. As I said at the beginning, it has been our regrettable custom in the past to neglect officially the cultural heritage of the alien peoples whom we have administered. For long enough, we neglected our own cultural heritage, and in important respects we are still neglecting it. But our Colonial record, so good in many ways, has been almost inconceivably defective in this matter. Look at Cyprus, for example, where our neglect obtruded upon the public eye and became so scandalous twenty or thirty years ago that special steps were taken at last to rectify it. If I am told that the difficulty generally is one of finance, my reply is 'Humbug.' It is not primarily or mainly a question of finance. It is a question of intelligent understanding on the part of our administrators, whether British or native, and of the deliberate and systematic informing of local opinion. The financial issues are relatively tiny. Let us conclude by putting down categorically the principal desiderata. (I am speaking now of 'monuments' and archaeology, not of archives.)

The foremost need is for an adequate Antiquities Law in each territory. The inadequacy of most of the antiquities laws at present extant would be easily solved by the issue of a pattern from the Colonial Office itself. That pattern would not, of course, itself have legal validity, but it would guide territorial legislatures in the absence of local experience, and could readily be modified to meet local circumstance. These modifications apart, a substantial uniformity in our antiquities ordinances throughout the Commonwealth would have obvious advantages.

Secondly, since a neglected law is worse than no law at all, it is essential to have a machine, however small, for the impletion of such an ordinance. The task of interpreting and applying an antiquities law is a highly specialised one, involving historical, archaeological and architectural knowledge of no mean order. It is *not* a task which can be left to the unguided discretion of governors and district commissioners. But it is precisely here that Finance Departments are wont to step in and hold up the advance.

What in fact are the minimum needs of a territorial archaeological department? I should say that they are, first, a *director*, who must be an individual of some skill and attainment if the department is to operate at all. Secondly, he must have

one or more assistant-inspectors, who will normally be of native origin. Thirdly, he must have a skilled native foreman, competent to supervise a multitude of operations, preservative and other, in the field. Fourthly, he must have a museum-cum-store and a trained curator. There is a cadre officer-staff, based upon the assumption that the Public Works Departments will co-operate where major structural conservation is in question. It will be admitted that the demand is not an extravagant one, particularly when it be recalled that certain of the charges are already incurred by many territories—e.g. for museums—in an ill-organised way. In other words, only a part of the budget would comprise entirely new items.

But even if the provision of this small staff does in fact strain available finance, I maintain that the charge must nevertheless be undertaken as a high priority. If necessary, something else must go. What the French can do, we can do. It is as essential, for example, to give the African a proper appreciation of his cultural heritage as it is to teach him to build a bridge. And let me again remind you that the time is short. If we devolve full political responsibility upon a colonial people without acclimatising it adequately to these cultural values and training it in the practical responsibilities which they involve, we shall fall seriously and culpably short of our duty. I repeat that Curzon set a cultural standard for India that ensured for generations an honourable esteem for his name amongst those to whom his political imperialism has been, is, and will be anathema. To put it at its lowest, a proper care for a cultural heritage is a political asset of no mean or negligible order. Again, regard the French example.

And now a last word or two. I have discussed mainly the official aspects of this problem, and it is their urgency that I would leave with you as my message. But I should not perhaps pass over without comment the last of the six Resolutions of the 1953 African Conference. You may remember that this Resolution urged the establishment of a School of History and Archaeology in East Africa on the analogy of the existing British Schools at Rome, Athens and elsewhere. Like our universities, those schools are remarkable institutions, financed largely by the State yet jealous of their autonomy. They are invaluable as centres of research, as magnets to the aspiring student, and, not least, as cultural embassies in the capitals in which they are situated. A year ago there were good grounds to hope that the Kenya Government might make the old Portuguese castle, Fort Jesus, at Mombasa, available for such a purpose ; and there can be no doubt that an institute of the kind at Mombasa could be a potent factor in developing facilities for research in East Africa. Unhappily, delays which have nothing to do with the present tragic events in Kenya have compelled the Government to cancel or defer its offer and to retain the Fort for the time being as a gaol. But the scheme, or something like it, must go ahead if historical studies are to be firmly founded in these territories and students, both native and foreign, attracted to systematic work. At the same time, I would discriminate clearly between this need and those other overdue measures of conservation to which I have devoted most of your time. There can be no two opinions as to their relative urgency.

My address has dealt with distressful matters, in the sense that our own shortcomings, unlike the foibles of others, are liable to weigh, and should weigh, depressingly upon us. But I at least am not a pessimist. I am satisfied that the present Colonial administration, granted that it is constantly preoccupied with matters of life and death, is well-intentioned in these things. It remains to ensure that the right path is paved with these good intentions, and that we do not longer loiter over the work.

THE RELIEF OF PAIN

Address by

PROF. A. D. MACDONALD

PRESIDENT OF SECTION I

AT a time which Dale has called the golden age of therapeutics, when chemotherapy is making one spectacular advance after another, it is still important to remember how much of medicine is concerned with the treatment of symptoms. The relief of pain will of course follow the cure of the disease, but this may be slow while the pain is urgent and the aetiology still obscure. Analgesics may be defined as drugs which reduce or relieve the sensation of pain without producing loss of consciousness or parallel depression of other senses. Thus general anaesthetics, while used at times, as in labour, for escape from pain, are not true analgesics nor are they desirable in the everyday treatment of pain.

Local anaesthetics are also now of established value in the treatment of such localised trouble as fibrositis, and skilled and experienced practitioners can often give remarkable and sustained relief by such injections. There has been some use of local anaesthetics intravenously in the relief of pain. This is difficult to justify pharmacologically, for these drugs, apart from cocaine, which is manifestly unsuitable for such application, are rapidly cleared from the blood-stream and fail to follow the Hughlings Jackson 'law' in their action. Instead of the usual descending paralysis produced by narcotics —descending in the sense that the most recently developed and highly specialised functions of the central nervous system are the first to be depressed—intravenous local anaesthetics are quite liable to paralyse respiration in doses which scarcely affect spinal reflexes. It is true that analgesics similarly break the 'law.' Adrian (1947), while doubting whether pain is as much appreciated at the level of the thalamus as Head maintained, agrees that 'Pain needs no learning to increase its potency. This must be due to a direct effect on the basal ganglia.' Analgesics should, therefore, act at this level as well as on the cerebral cortex, but while there is presumptive evidence of selective effects, there is need for something like tracer technique to establish real localisation of drug action. Probably by introducing a radioactive isotope into the analgesic molecule, the concentration of the drug could be estimated in the tissues even in great dilution, and its fate followed. So far such studies have failed to be impressive.

Methods of testing the potency of analgesic drugs are numerous. Where long series of related compounds have to be compared, some animal screening is first necessary. The promising drugs may then be assessed comparatively on human volunteers, as by Jackson (1952), Prescott *et al.* (1949), Hewer and Keele (1948). Probably even such results in man require checking by experience in the actual relief of human suffering before much weight can be attached to them.

Screening tests have been carried out on all sorts of laboratory animals, and a variety of painful stimuli have been tried, of which heat in one form or another has been most popular because of the precision with which it can be repeated and measured. It is important that the intensity and duration of the stimuli should be such as to cause no tissue damage, since such would inevitably lead to changes in threshold. Fortunately, there is no need for stimuli of such severity. In assessing possible analgesic value the toxicity of the proposed remedy and any side actions which it may elicit must be taken into account, but it may not be possible to appreciate such considerations

till clinical trials are instituted. In man, heat may be applied, till pain is felt, by the use of a resistance coil or by focusing the light of a powerful lamp on a fixed area of blackened skin (Hardy *et al.* 1947). Other methods preferred by some workers for the study of analgesic action consist of assessments of the modification of the pain which the drug affords when pain is elicited by injections of hypertonic saline or by muscular contraction in an ischaemic limb. These and other methods are listed with numerous references in the chemical review of the synthetic analgesics by Bergel and Morrison (1948). There is abundant evidence that there is liable to be a large psychological element and a substantial individual variation in all such assessments, so that rigorous controls are necessary.

While analgesics have been shown to be very active when applied to the mid-brain directly, in quite small doses, the precise mechanism of their action is still unknown. Unlike most narcotics, analgesics have little effect on the oxygen consumption of the brain slice and on cholinesterase systems, but they may block the availability of amino acids or other essential metabolites to certain nerve-cells.

Opiates

Opium has been used in the relief of suffering for at least three thousand years. It is nearly 150 years since morphine was isolated from opium. Yet as recently as 1938 Fourneau claimed that morphine and a few of its derivatives could alone be considered true analgesics. The coal-tar antipyretics, widely used for certain nerve and muscle pains, seemed so far behind opium in the relief of pain associated with organic disease that Fourneau suggested they be called ' antalgics,' while he called cannabis and the related tetrahydrocannabinols ' euphorigenics,' euphoria being the most striking of the effects they normally produce. The hemp drugs have a definite analgesic action in animals but, rather curiously, increase in dosage does not appear to enhance this analgesic effect (Davies, Raventos and Walpole, 1946). They have a long history in therapeutics, and the synthetic compounds may bring them again to the fore (Macdonald, 1941 ; Avison, Morrison and Parkes, 1949). While cannabis is scheduled with the dangerous drugs and is known to be a frequent drug of addiction, it is claimed to be free from any such risk in therapeutics, but its applications there have been so wide that one hesitates to accept them. Its value in the amelioration of mood in mental disorders appeared to be promising, but most pharmacologists would regard it as superfluous.

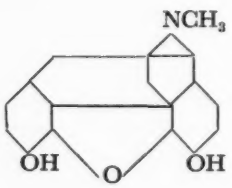
The literature of the opiates is enormous, and no attempt to review it can be included here. The United States Public Health Services cover it in two large volumes (Kreuger, Eddy and Sumwalt). In experimental animals, the action of opium and of total opium alkaloids is substantially the action of the morphine they contain. Potentiation by the other alkaloids is hard to demonstrate, and so is potentiation by neostigmine, though this has been claimed. Such use of a cholinesterase inactivator is the more revolutionary in that for many years it has been customary to give atropine or hyoscine with morphine to reduce its side actions if not enhance its analgesic effects. Many experienced physicians use morphine almost exclusively.

Many workers have published tables in which the analgesic activity of a series of drugs is assessed numerically in terms of morphine. Such tables may be misleading. Here let it be stated that the ideal analgesic is not established by a claim that it is, say, ten times stronger than morphine. Morphine is usually strong enough. What is wanted is a drug which has morphine's anxiety- and pain-relieving qualities, together with less or none of its undesirable side actions. Morphine lies open to criticism in that it is liable :

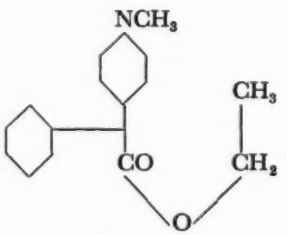
(1) to depress respiration ;
(2) to produce nausea and vomiting ;
(3) to be constipating ;
(4) to produce tolerance and the chance of addiction ;
(5) to increase itching and skin irritation ;
(6) to be dangerous in susceptible subjects and young children.

A substance like codeine (methylmorphine), though it has only one-sixth or less of the analgesic power of morphine, is often preferred because of its relative

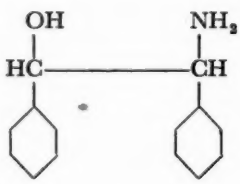
freedom from these side actions. It has a particular reputation in the relief of cough, yet in a recent investigation in Glasgow, codeine had none of the value of morphine or heroin in preventing experimentally-induced cough (Hillis, 1952). Cass and Frederik (1953), in concluding that 4 mg. dextromethorphan is an effective antitussive, found that 17 mg. codeine was better, in an elaborate clinical study. Diamorphine (diacetyl-morphine) on the other hand, though a powerful and reliable analgesic, leads to habit formation so frequently and so quickly, that its manufacture and importation were forbidden in the United States and this lead has been widely followed. There is a considerable movement to ban it similarly here, because of recent evidence of increased consumption and increasing addiction in various other countries, and the British Pharmacopoeia and National Formulary no longer include it. Monoacetylmorphine is about four times as active as morphine when assessed by the methods of Hardy and Wolff (irradiation



Morphine



Pethidine



β-Hydroxy-a : β-diphenylethylamine

of blackened skin) or Smirk and Alam (pain produced by exercise of ischaemic limb), yet produces only a 'moderate euphoria,' much less nausea and vomiting, and allows of increased voluntary muscular effort in the presence of severe pain.

Levorphan, dihydromorphinone (Dilaudid), and methyldihydromorphinone (Metopon), are in the diamorphine class or better as regards relieving pain. They are effective in the late stages of malignant disease, and good by mouth, but their actions are short and they may certainly provoke addiction.

The attempts to modify the structure of morphine in such a way as to relieve it of its side actions but retain its analgesic value cannot be said to be strikingly successful, though much research has been directed to this problem. Where the chemist has been successful in increasing the analgesic action by his manipulations, his product has usually shown increased toxicity, though not always a parallel increase in the two actions. As a rule the increase in potency is accompanied by a decrease in duration of action. Many regard the search for an analgesic which is not a potential drug of addiction as futile, but there is a sustained effort to find a morphine substitute which is less depressant to the respiratory centre. The failure so far to synthesise morphine, in spite of attempts by many distinguished chemists, has probably been a major difficulty, but Gulland and Robinson (1924–5) here, and Grewe (1947) in Germany, have made marked progress. There is an alternative method of investigation—to try and identify the basic part of the morphine molecule with which analgesia is associated and then test various chemical modifications of this for potency.

This is the technique so successfully followed by Dodds in the development of the synthetic oestrogens. In the case of morphine he believed that diphenylethylamine was the core of its efficiency, and tested 18 compounds by the Hardy-Wolff and other techniques (Dodds *et al.* 1944). Peak activity was found in hydroxydiphenylethylamine, which gave 'complete relief in doses of 200 to 400 mg. four hourly.' At first this looked promising, but later it appeared that many forms of pain were uninfluenced by these drugs, though in malignant disease they were claimed to be of special value where pain is due to pressure. In comparative tests in animals this compound shows no significant activity.

Pethidine

Pethidine was introduced (Eisleb and Schaumann, 1939) not as an analgesic but as a spasmolytic. Its original name was Dolantin, and it was introduced here as Dolantal, in America as Demerol. Its ability to relieve pain was discovered later, and its rather remote chemical relationship to morphine suggested. Though as an analgesic it seemed to be in the codeine rather than the morphine class (Woolfe and Macdonald, 1944), the facts that it had any real pain-depressing activity and that it provided a convenient molecule for modification by the synthetic chemist gave a fresh impetus to research in analgesia. Pethidine was the best of Eisleb's compounds as assessed by Schaumann (1940). A long series of related compounds synthesised by Bergel and his team was assessed pharmacologically by Macdonald and Woolfe (1946). Some had a longer action on mice than pethidine and some had a slightly stronger action—notably 2-methylpethidine—but although certain of these derivatives received some clinical encouragement (Glazebrook and Brantwood, 1945), the differences were not sufficiently great to be very important. Since then an ethyl ketone Hoechst No. 10720 (ketobemidone) has been claimed to be ten times as active as pethidine, and has had a promising first clinical trial (Kirchhof, 1949), but its future value is limited because it shows the dangers of morphine as a drug of addiction.

Pethidine is of established value in relieving the pain of labour, and this may be related to its spasmolytic action. A recent series of 800 cases has been reviewed by the committee, on which I had the honour to serve, set up by the Medical Research Council. This committee has concerned itself largely with the development of a suitable inhaler for the administration of trichloroethylene in a concentration of 0·4 to 0·6 per cent, and several such instruments have been developed. The report of the committee has not yet been published, but the conclusions are—

(i) that trichloroethylene is to be preferred to nitrous oxide for use by midwives if administered from suitable and carefully maintained inhalers;

(ii) that these inhalers can be used together with pethidine, without increasing the risks in either mother or child, except that the labour may be prolonged.

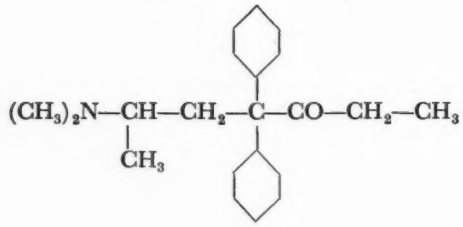
Methadone and Related Compounds

Methadone (Hoechst 10820), also known in Britain as amidone, miadone and physeptone, and in America as methadon, adanon and dolophine, was revealed during the investigation of I.G. Farbenindustrie at the end of the war. This compound again stimulated a fresh outburst of research. It was found to be at least as powerful an analgesic as morphine, yet less hypnotic. Its use, however, is often complicated by prolonged nausea and vomiting, and to-day it may be more important as a source of new compounds, which may retain analgesic efficacy, yet be free from these unpleasant side actions. Of such, isoamidone, which was discarded by the original Hoechst workers, has had favourable reports both here and in America (Luduena, 1948), and 2-dimethylamino-3-acetoxy-4:4-diphenylheptane and the 2-morpholinopropyl compound (C.B. 11) are relatively free from unpleasant toxic effects.

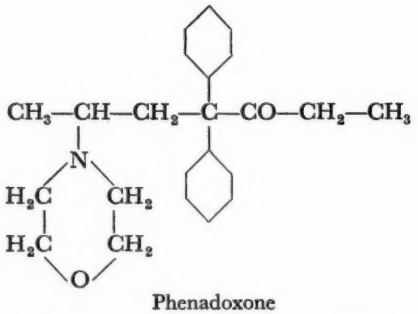
It is a pity that the latter (Phenadoxone, 'Heptalgin') was at one time advertised as ' activity six times that of morphine,' even if certain animal tests confer such a ratio. It is much more important to be assured that the acute toxicity is relatively much lower and that side effects, apart from mild drowsiness with full dosage, are negligible. Relative freedom from serious respiratory depressions and constipating action is freedom indeed.

Wilson and Hunter (1948), comparing methadone, phenadoxone, and pethidine, found that 5 mg. of methadone relieved ischaemic pain in only six of ten subjects, whereas 5 mg. of phenadoxone relieved nine of the ten. Both were better than 50 mg. of pethidine, but this was strikingly more euphorigenic than the newer drugs. Phenadoxone, however, has not proved as reliable an analgesic as had been suggested by the early experiments.

The optical isomers of methadone have been prepared (Thorp *et al.* 1947) and



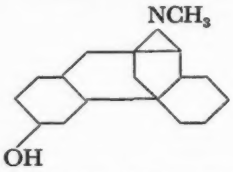
Methadone



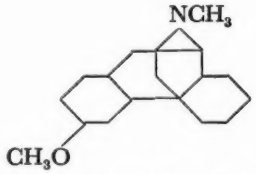
Phenadoxone

compared with the racemic forms (Thorp, 1948). The laevo-isomer is responsible for the effects of methadone on the central nervous system, while the dextro compound shares equally in the spasmolytic, local anaesthetic and toxic actions on the circulation (but not on the respiration) in experimental animals.

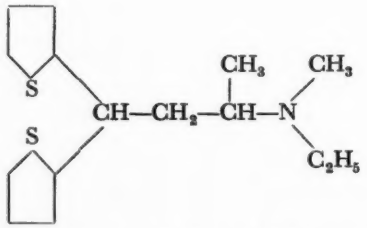
Amongst more recent developments reference should be made to the morphinans and dithienylbutenylamines. The morphinans may be regarded as a simplification of morphine, and 1-3-hydroxy-N-methylmorphinan (Levorphan, 'Dromoran') is a powerful and reliable analgesic, but carries most of morphine's disadvantages.



3-hydroxy-N-methylmorphinan
(Levorphan, 'Dromoran')



d-3-methoxy-N-methylmorphinan
(Dextromethorphan)



Ethylmethylamino-1,1-(2′-thienyl)-but-1-ene
(IC 50, NIH 5145)

Its action lasts longer, and it is perhaps better than morphine by mouth. It is interesting to find, as in the amidones, that analgesic and narcotic actions are limited to the laevo-isomers, while d-3-methoxy-N-methylmorphinan (dextromethorphan), though neither analgesic nor a potential addictive, has well marked effects on cough (Cass and Frederik, 1953).

In a survey of the dithienylbutenylamines (Flintan and Keele, 1954), these compounds are regarded as better than pethidine but inferior as analgesics to morphine in the relief of post-operative pain. These drugs produce euphoria and must be regarded as potential drugs of addiction (Isbell and Fraser, 1953). They are less liable than morphine to produce nausea and vomiting, but more hypnotic, and ineffective when taken by mouth, Their actions are antagonised by nalorphine.

Few important recent developments have taken place in Fourneau's 'antalgics.' Amidopyrine is probably still the most potent but has lost favour because of its occasional effects on the bone-marrow. Aspirin is still the most widely used, yet

phenacetin is regarded by critical observers as a more effective drug, though little used by itself. These two, in combination with a little codeine, at present enjoy an enormous vogue, but there is some doubt whether the claimed ' potentiation ' in such mixtures will bear pharmacological scrutiny. It is interesting to find that rheumatoid arthritis, on a long-term view, responds as satisfactorily to aspirin as to cortisone (M.R.C. and Nuffield Foundation Report, 1954).

Much interest has been taken in phenyl butazone (' Butazolidin '), in the relief of rheumatic and arthritic pain. The drug has a bad reputation for causing leucopenia or worse, but sometimes this appears to have been due to its combination with other drugs which can affect the bone-marrow. It seems clear that it acts as a long-lasting analgesic rather than as a drug which affects the cause of the disease.

With methyl pentynol, another recent introduction, no analgesic action is produced by ordinary doses. Its main advantage is that its narcotic action is slight and brief, yet it is claimed to have remarkable effects in restoring morale and maintaining confidence—e.g. before resigning oneself to the hands of the surgeon or dentist.

The barbiturates, which at one time were claimed to be analgesic as well as hypnotic, have failed to live up to any such claim except in anaesthetic doses—the use of the shorter-acting compounds as intravenous anaesthetics is undoubtedly a major advance. The barbiturate-antalgic combination, so bitterly opposed in the past by Willcox, has been restricted by the inclusion of barbiturates in Schedule IV of the Poisons Act and by the wide publicity given to the toxic risks of amidopyrine, which is similarly scheduled. Wayne (1947) has recently emphasised the risks involved in the abuse of such drugs.

Most new analgesics, whether related to morphine or not, have been introduced as ' free from morphine's tendencies to produce tolerance and addiction.' None has seriously stood up to critical tests of such claims—perhaps it is too much to expect of an analgesic. Many have claimed, as indeed De Quincey did, that addiction is something which follows relief of pain, or faith in such relief. But addiction where there is no background of painful disease is not so rare, although one asks oneself about addiction risks as a topic of primary importance in relation to every new synthetic analgesic. Again, such drugs as the barbiturates, which must be accepted as potential drugs of addiction, are only analgesic in anaesthetic doses.

If not to be assessed or assayed on analgesic potentialities, can we establish any parallel between euphoria and addiction? Euphoria is a form of stimulation of the C.N.S., although we still teach that alcoholic euphoria is better explained as a depression of cortical inhibitions and of course euphoria may either follow pain relief, or be a factor in such relief in the case of a drug like morphine.

The fashionable pharmacology of to-day regards drugs as enzyme inhibitors—what is often called ' Acetyl Choline (A.Ch.) and all that ' ! On isolated enzymes, morphine is found to be a strong inhibitor of cholinesterase—the enzyme which limits the intensity and duration of the action of the A.Ch. which is liberated at parasympathetic or rather cholinergic nerve endings. Now the adrenal gland liberates its adrenaline under A.Ch. stimulation, and morphine causes liberation of adrenaline. Peso indeed has suggested that analgesia may be an adrenergic process, and amphetamine, an anti-amine-oxidase in part, is known to potentiate the analgesic action of morphine. Euphoria, however, is not necessarily related biochemically to adrenergic activity.

Light on the nature of the enzyme action may be shed by a study of antagonists to drugs of addiction—such as nalorphine (n-allyl-nor-morphine) against morphine and disulfiram (' antabuse ') against alcohol. The rapid and sustained relief nalorphine affords from the symptoms of acute morphinism is striking, and suggests the existence of competitive displacement between the two drugs in the enzyme-coenzyme system. But the action is less specific than at first appeared—in addition to antagonism to opiates and drugs related to the opium alkaloids, nalorphine has been shown to be effective against methadone and pethidine, and more recently (at a higher dose level) possibly even in acute barbiturate poisoning. I believe that the use of disulfiram is no longer

advocated by experts, but it clearly produces its effects by modifying the fate and clearance of alcohol.

To extend the professional pharmacologist's tribute to humoral theory, two other observations may be quoted :

1. In addiction (to morphine) balance, physostigmine (eserine) induces withdrawal symptoms. Is this because of its protection of brain cholinesterase from the inhibitory effects of morphine, or because of a more far-reaching upset of enzyme balance ? We do not know.
2. Histamine, it has been suggested, is an anti-morphine factor which might be responsible for the abstinence syndrome, for many of the symptoms can be duplicated by histamine administration and it has been established that antihistamines are of value in the control of morphine—withdrawal symptoms.

In the elaborate American experimental studies of drug addiction in both men and infra-human species, it seems that the three principal components of the syndrome are agreed to be :

1. Psychic dependence ;
2. Physical dependence ;
3. Tolerance.

Seevers, in his studies on monkeys, maintains that physical dependence is the only reliable criterion, for, of course, psychic dependence is harder to recognise and still harder to measure even in the monkey than in man, and impossible to assess in many species. Seevers, while admitting that there is a rough parallel between degrees of addiction and tolerance established to morphine, pethidine and methadone, is clear that tolerance studies alone are inadequate for the assessment of addiction liability.

Wikler has stressed the difficulty of separating psychic and physical dependence, since tachycardia, vomiting and even fever may be ' psychic ' in origin and ' purposive ' behaviour may have a ' physical background.' He maintains that only by the use of chronically decorticated and spinal animals can a differentiation be made, and has developed techniques at Lexington for such a study of drugs of addiction.

Isbell and Fraser, in their studies of human addiction, justify the use of morphine addicts for such investigations and maintain that physical and psychic dependence are equally important. They use four methods :

1. Administration of single doses for the detection of euphoria,
2. Effect of single doses on abstinence symptoms in addicts,
3. Substitution for morphine in patients strongly addicted to morphine, and
4. Direct deliberate addiction production to the drug under study, in volunteers abstinent from morphine for three months or more. This type of experiment, however, is so prolonged and so demanding on the staff that it is bound to be limited, for each patient may require up to seven attendants for three to seven months.

Isbell (1948) maintains that the addiction liability of methadone is nearly as great as to morphine, while isomethadone is only in the codeine class. It seems clear from these studies, into which millions of dollars have been poured from public funds, that animal experiments are of limited and highly controversial value in this field, and the ethics of experimental production of addiction in man—even in the ' confirmed ' morphine addict—are still open to question.

But the advances in this analgesic field in the past twenty years are full of hope and promise, and whether an approximately ideal drug can be provided by the acetylated alcohols, corresponding to the ketones of the amidone group, by some other derivatives or in some quite different way, it will surely be found in due course.

References

Adrian (1947) : *Practitioner*, **158**, 76.

Avison, Morrison & Parkes (1949) : *J. chem. Soc.* pt. 2, 952.

Bergel & Morrison (1948) : *Quart. Rev. chem. Soc.* **2**, 349.

Cass & Frederik (1953) : *New. Eng. J. Med.* **249**, 132.

Davies, Raventos & Walpole (1946) : *Brit. J. Pharmacol.* **1**, 255.

Dodds, Lawson & Williams (1944) : *Proc. roy. Soc.* B, **132**, 119.

Eisleb & Schaumann (1939) : *Dtsch. med. Wschr.* **65**, 967.

FLINTAN & KEELE (1954): *Brit. J. Pharmacol.* **9**, 106.
FOURNEAU (1938): *Chim. Ind.* **39**, 1043.
GLAZEBROOK & BRANTWOOD (1945): *Lancet*, **249**, 528.
GREWE (1947): *Z. angew. Chem.* **59**, 194.
GULLAND & ROBINSON (1924–5): *Proc. Manch. lit. phil. Soc.* **69**, 79.
HARDY, WOLFF & GOODELL (1947): *J. Clin. Invest.* **26**, 1152.
HEWER & KEELE (1948): *Lancet*, **132**, 683.
HILLIS (1952): *Lancet*, **262**, 1230.
ISBELL & FRASER (1948): *Ann. N.Y. Acad. Sci.* **51**, 108.
ISBELL & FRASER (1953): *J. Pharmacol.* **109**, 417.
JACKSON (1952): *Brit. J. Pharmacol.* **7**, 204.
KIRCHHOF (1948): *Fed. Proc.* **7**, 234.
KREUGER, EDDY & SUMWALT, U.S. Pub. Hlth. Service Supp. 165.
LUDUENA *et al.* (1948): *Fed. Proc.* **7**, 241.
MACDONALD (1941): *Nature, Lond.*, **147**, 167.
MACDONALD, WOOLFE, BERGEL, MORRISON & RINDERKNECHT (1946): *Brit. J. Pharmacol.* **1**, 4.
Medical Research Council and Nuffield Foundation Report, 1954: *Brit. med. J.* i, 1223.
PESO (1939): *Giurnal anest. anal.* **5**, 128.
Pharmaceuticals: Research and Manufacture at I.G. Farbenindustrie, B.I.O.S. Report No. 116. (H.M. Stationery Office, 1945), 51–2, 65.
PRESCOTT, RANSOME, THORP & WILSON (1949): *Lancet*, **256**, 340.
SCHAUMANN (1940): *Arch. exp. Path. Pharm.* **196**, 109.
SEEVERS (1948): *Ann. N.Y. Acad. Sci.* 51.
THORP (1949): *Brit. J. Pharmacol.* **4**, 98.
THORP, WALTON & OFNER (1947): *Nature, Lond.*, **160**, 605.
WAYNE (1947): *Practitioner*, **158**, 514.
WIKLER (1948): *Ann. N.Y. Acad. Sci.* 51.
WILSON & HUNTER (1948): *Brit. med. J.* i, 553.
WOOLFE & MACDONALD (1944): *J. Pharmacol.* **80**, 300.

RECENT STUDIES IN THE PSYCHOLOGY OF THINKING

Address by

PROF. L. S. HEARNSHAW

PRESIDENT OF SECTION J

I. *Introductory*

IN the thirty-odd years since the establishment of this section no President has yet delivered an address specifically on the psychology of thinking, though Wolters at Blackpool in 1936 more than once referred to it. Partly, no doubt, this omission has been due to the fact that our Presidents have been judicious and modest men, well aware of the limitations of their nascent science, and of the critical acumen of their audiences. But partly it has been because only in recent years has the psychology of thinking, which momentarily held the stage in the first decade of this century, revived as a serious and active interest for a considerable number of psychologists. In a brief paper which I delivered at the Liverpool meeting of the Association last year (Hearnshaw, 1954) I suggested that there were a number of reasons for this trend, and that contemporary work in psychopathology, child psychology, and the experimental investigation of learning and perception, together with various developments in physiology, biology and philosophy were all converging in a way that almost forced psychologists to reconsider the whole question of thinking. I have chosen this topic because there is among psychologists and others a growing sense of its importance, a growing sense that, however complex and difficult, it is a topic that can profitably be investigated, and indeed must be investigated if progress in other areas of psychology is not to be held up. My aim is to present a sort of interim report on some of the more recent work and speculations of psychologists, in the field of normal psychology and tentatively to pick out certain trends which may possibly be significant. I can lay no claim to any particular originality in what I have to say, and am well aware that there are many others far better equipped than myself to make such a report, in particular, of course, Professor Humphrey of Oxford, to whom we all are indebted for two very valuable books on the psychology of thinking (Humphrey, 1948, 1951).

May I begin with a brief apology for the study of thinking by psychologists? It is quite commonly believed that to construct a psychology of thinking is not merely an overweeningly ambitious enterprise, but in principle a mistaken one, which might even have dangerous results. For the psychologist, it is held, must aim to show how thought processes are determined by irrational influences and material forces, and hence necessarily undermine our faith in the rationality and freedom of human thought. As the philosopher Collingwood put it, ' the mind regarded in this way, ceases to be a mind at all.' (Collingwood, 1939.) The result is a degradation of thinking to the level of other natural processes, and a neglect of all its unique features. Such a prospect is greeted sometimes with amused incredulity by the layman, but more often with alarm or annoyance; while the philosophers, even if they do not all show Collingwood's detestation for psychology, without much difficulty bring their logical armament to the demolition of an absurd and untenable position. It is this kind of criticism which is, I believe, based on a fundamental misunderstanding of what psychology is trying to do.

The basic contention of the psychologist is simply this: thinking is the activity of an organism and does not occur *in vacuo*. It is indeed an extremely high level activity

in the sense that it depends upon the organised co-ordination of a vast number of subordinate activities. This high level activity is rendered possible only as the result of a long period of evolutionary and individual development, and it depends on the co-operation of favourable organic and environmental conditions. The two major aims of the psychologist are, therefore, first to study in detail the development of thinking, and if we may regard abstract, logical or mathematical thinking and artistic creation as the two most highly evolved types of thinking, to reveal the genetic foundations upon which these types of thinking rest. Also, of course, the psychologist is interested in describing and classifying all the less developed forms of thinking ; for just as many lower forms of life coexist with the higher, so many less developed kinds of thinking coexist with the higher flights of logical and creative thought—and indeed, as we all know, these higher flights are still comparatively rare. The second major aim of the psychologist is to study the conditions upon which thinking depends ; conditions which, if unfavourable, may distort thinking or completely prevent it. The study of thinking cannot be divorced from the study of a whole range of conditions, from organic and physiological conditions at one extreme, to complex personal and interpersonal, or social, conditions at the other. This, however, is not to deny the possibility of rationality in certain circumstances ; it is only to deny its universality. To these two aims there can be added a third : the psychologist hopes to illuminate and gradually to understand the *general* nature of the processes involved in thinking —which is quite a different thing from a precise determination and prediction of individual thoughts. Briefly put, the psychologist aims to study thinking as a capacity of organisms, its growth, conditions of functioning, structure and mechanisms. I believe that the psychological study of thinking so defined obviates all or most of the objections that philosophers and others have raised.

In this endeavour the psychologist has a twofold interest. It is an essential part of a systematic psychology. Those strange textbooks of psychology which devote at least one-third of their space to sensory physiology and another third to the learning of rats, are pitifully one-sided and inadequate. The older books, whatever their shortcomings of method, had at any rate a better sense of the scope and proportions of their subject-matter. The job of the psychologist is to build a systematic psychology which has the breadth of the older speculative schools with the objectivity and methodological rigour that the present-day demands. Above all this marriage must be effected in the study of thinking.

The second interest of the psychologist in the study of thinking is a practical one ; for the psychologist today is a professional practitioner as well as a scientist. As such he must be guided by some set of values. He is concerned for the proper growth and training of the intellect ; he aims to understand and to prevent the pathologies of thought, from the minor ailments of quirk and prejudice to the major delusions and disruptions of psychosis. This involves some kind of ideal of intellectual health, as perhaps we might term it, or as Aristotle put it long ago, referring it seems to much the same thing, of intellectual virtue : a state of perfect development in which intelligence as intellectual power is one and only one component. In a striking article written nine years ago, the American psychologist, Gardner Murphy described a principal goal of the psychologist's practical activities as ' The Freeing of Intelligence ' (Murphy, 1945). ' Perhaps the major contribution which psychology can make to this generation,' wrote Murphy, ' is to show how intelligence may be freed of the incubus which sits upon its chest, and to enable a free intellect to cut through the hideous confusion of today.' Can we venture to hope, then, that a psychology of thinking may provide us with the foundations not merely for the more limited task of assessing intelligence for which it is, of course, indispensable, but also for the wider and more important task of freeing it ?

In these introductory remarks I have attempted to indicate what the psychologist is trying to do in the study of thinking. I hope I have shown that it is not a subversive study which seeks to divest thinking of its characteristic attributes, but in so far as logical thought is

not only a formal pattern but also an activity of an organism, a necessary complement to the method of philosophical analysis. Let me now turn to my main theme, which is to discuss some of the recent work of psychologists.

II. *The Developmental Studies of Piaget*

Among this recent work I believe a very high place must be given to the developmental studies of children's thinking carried out by Piaget and his assistants, and I therefore propose to devote some time to its consideration. The comparative neglect of Piaget's more recent investigations by British and American psychologists is an interesting example of cultural insularity, for which, no doubt, there are several explanations. Piaget's published work falls into two well-marked groups (see bibliography). Between 1925 and 1932 he produced a series of five books on the language, thought and moral judgement of the child based on studies carried out in a Geneva kindergarten. These works were all translated into English, and it is by these older works that Piaget is still, more often than not, judged in English-speaking countries. Now these early books, as Piaget has himself openly confessed in his autobiographical study (Piaget, 1952), have several shortcomings both as to scope and method : his investigations, in his own words, ' had barely entered the preliminaries.' The second group of works, which were published since 1937 and are of an altogether more penetrating nature, are only just becoming familiar to English readers. Only four of some sixteen volumes have as yet been translated. No adequate and comprehensive account of Piaget's work has yet appeared in English apart from the translation of Piaget's own difficult and compressed lectures on *The Psychology of Intelligence*.

The importance of Piaget's studies is not merely that they constitute by far the most detailed account of children's thinking from the hour of birth to adolescence that has yet been made ; it lies much more in the comprehensiveness of their scope and their systematic theoretical organisation. It is not enough to describe and catalogue the childishness of children's thinking ; childish modes of thought are stages on the way to, and a necessary preparation for, the logical thinking of which the adult is capable. An outstanding characteristic of Piaget's studies is their grasp of the entire range of cognitive development from the sensorimotor matrix at the foundation to completely formal and abstract logical and mathematical systems at the apex. No account of thinking can be acceptable unless it is compatible with modern logical analysis, and unless it recognises that man's most rigorous thinking is mathematical thinking. Piaget's account meets this requirement ; it culminates in a systematic epistemology and a formal logic. It embraces, too, what is equally essential, that other important type of thinking, symbolic thinking, and illuminates not only the symbolic activities of childhood, but the general nature of symbolism and its relation to the emotional side of the mind and to psychopathology. It is without doubt the most comprehensive account of the development of thinking that we yet possess, and it links, as no other account so adequately does, the two poles of biology and logic : a task for which Piaget is admirably equipped : for he commenced life as a zoologist and has developed into a philosopher.

The basis of Piaget's work is, of course, the extremely detailed observations and the numerous experiments which he has carried out both on his own children and in Geneva schools. His observations on his own children commence a quarter of an hour after birth ! It is not recorded why Piaget missed the first quarter of an hour ; but here at least is a chink for future research workers. These observations, which are fully reported in his books, in themselves constitute a remarkable record. Piaget, however, does not merely observe facts and record them ; he interprets, and is quick to spot their possible significance. He gives us no mere piles of unorganised data, but a theoretical framework which holds together and orders them. Piaget's work has not infrequently been criticised by English-speaking psychologists for its methodological looseness and its absence of statistical control. I rather regard this, in the present stage of psychology's

development, as one of its merits. The richness of Piaget's first-hand observations, and the broad and penetrating yet flexible theoretical framework into which these observations fit, seem to me to show a truer appreciation of the nature of science than is shown by over-zealous attempts at quantification and statistical analysis, or by the rigidly formal deductive systems so often favoured by contemporary Anglo-American psychologists. Before we measure and before we systematise, let us observe and reflect—provided of course, that we do so with critical rigour.

I can give no adequate idea in the brief time available of even the key concepts of Piaget's psychology. It is, of course, fundamentally a genetic psychology. The study of development is the simplest way in which the complex, interlocking structures of the adult mind can be broken down and analysed. What introspection has failed to do, and must necessarily fail to do, genetic studies may achieve. But further than this the genetic approach is necessary because the human mind is essentially historical. No behaviour patterns whatever can be detached from the historical context of which they form part. Psychological development as such begins only when past experience influences present behaviour. It is a continuing and continuous process in which the present is always actively assimilated to schemata organised in the past. Thinking cannot be regarded merely as the association of ideas, or as the eduction of relationships, or as insight into structure, as various schools of psychologists have regarded it ; the immediate act of association, or of eduction, or of insight—the capacity to see the point, to judge correctly or to solve problems—all these are only possible as the result of a long historical process of assimilation and organisation. Obvious when applied to our higher capacities—all would admit that some scientific training is a prerequisite for scientific thinking—this has been ignored when it comes to everyday problem solving, and denied when it comes to the basic categories of experience. What Piaget has shown with a wealth of detail is that even the fundamental aspects of experience—thinghood, space, time, number and so on—are historical achievements for each one of us. There are no frames of reference which are wholly *a priori* and uninfluenced by experience as some philosophers have supposed. The fundamental categories of thought are themselves constructed step by step as the result of sensori-motor activity during the early years of life, at first slowly and gropingly, then more rapidly as a result of socialisation and the acquisition of language. An analysis of adult thinking, whether carried out introspectively or objectively, misses the background schemata, the organised and now unconsciously operating experience, which alone renders such thinking possible. To study the formation and maintenance of these organised schemata is, therefore, a most important aspect of the psychology of thinking.

In the process of development from birth to maturity Piaget insists that there is a functional continuity. New structures develop partly, no doubt, as a result of maturation, but the underlying processes are the same. The basic process Piaget terms ' assimilation ' : the new is taken up into and assimilated to previously organised schemata. These schemata are not unlike the configurations of the Gestalt psychologists, but they are conceived as dynamic and developing. Assimilation is not merely reproductive ; by a process of generalisation schemata reach out beyond themselves and display novelty. They are corrected and differentiated by the need for accommodation to reality. Broadly speaking Piaget distinguishes three main stages of intellectual development sensori-motor activity, egocentric representational thought, and rational or operational thought. The last stage is intimately tied to, and dependent on, the first not only by the continuity of development, but also by the identity of the processes involved throughout. The concepts of the rational level spring from the schemata of the sensori-motor level. Our mature capacity for judgement is an highly evolved instance of the assimilatory activity which is also present at the very beginning of intellectual development.

There are two critical points in this development which mark the beginning of new levels ; at round about the age of eighteen months representative activity begins. There is evidence then for the

existence of ideas or images detached from present reality: this is shown for instance by delayed imitation and by simple symbolic play, in which the body is used to produce an action foreign to the present context. Representation is characterised by the fact that it goes beyond the present and extends the field of adaptation; in particular it extends it in the temporal dimension. Piaget traces in some detail the process of transition from the sensori-motor to the representational level. Representation is an internalised action and still remains an action. Thinking is at no stage and in none of its forms a purely passive receptive process. Piaget, for instance, makes the interesting suggestion in support of which he provides some evidence, that the image is an interiorised imitation. This perhaps becomes plausible if we remember that perception itself is an active integration, partly learned, and not merely a passive mirroring of external reality. The development of representative thought is, of course, much assisted by language and by social intercourse, but it cannot be wholly accounted for by them. There is also an internal evolution of thinking itself, without which neither language nor the communication of ideas would be possible. An important feature of Piaget's system is that he disagrees with the fashionable philosophical identification of thought and language, and shows experimentally that this does not hold in the child.

The second critical point in the development of thinking occurs in general between the ages of seven and eleven, and is marked by the systematisation and decentralisation of thought. Systematic logical structures, which Piaget calls groupings, gradually supplant the partial, inconsistent, intuitive notions of the egocentric stage of development. Piaget shows how concepts become organised in classificatory systems, how temporal relations are merged in the notion of a single time and spatial relations in a single conventional space; while numbers become systematised in a way that permits mathematical treatment. With these groupings come objectivity, coherence, and the appreciation of logical necessity. The rise of this new level of rational or, as Piaget terms it, operational thinking, is a landmark in human development. The formal logic which marks this stage does not adequately describe the earlier stages of thinking; it describes the final equilibrium towards which the earlier stages tend. Piaget has recently expounded his views on the relations between logic and psychology in his Manchester lectures —the only one of his books to be published first in English—and I shall not attempt to summarise them. In showing how the broken bonds between logic and psychology may be reunited Piaget has perhaps performed his most notable service both to psychology and to philosophy.

In this very brief survey of Piaget's work it has not been possible to convey any conception either of the wealth of the observational data upon which it rests or of the intricacy and range of his theoretical concepts; and it is precisely in the marriage of data and theory that its main strength lies. His work will bear comparison with the great speculative systems of the past; while at the same time it shows that respect for 'irreducible and stubborn facts' which Whitehead, quoting William James, pointed to as a characteristic trait of the modern mentality. The facts Piaget amasses are, of course, behavioural facts: what he has not attempted to do in detail is to link his observations to any neurophysiological base; nor does he speculate, as Hebb does, as to what may be going on in the brain and nervous system as the child learns and develops. There is reason to suppose, however, that just because of its link with logical calculus Piaget's work might eventually be translatable into neurophysiological terms. Indeed in his latest articles Piaget has begun to show a lively interest in such topics as cybernetics and the construction of computing machines. That, however, is a matter which he has not yet explored in detail; so let me leave Piaget at this point; and also let me pass over other contemporary developmental studies, in particular studies in the field of animal learning and intelligence, which after being bogged down for some time in a theoretical controversy between connectionist and Gestalt viewpoints, shows signs of new inspiration and progress. I shall refer briefly, later, to Harlow's work with primates: meanwhile I turn to the second main topic in the

psychology of thinking, the investigation of its concomitant conditions.

III. *Concomitant conditions*

Thinking is a highly organised and delicately balanced activity which depends upon the co-ordination of a large number of subordinate activities under favourable conditions. The delicate balance is easily tipped ; the co-ordinations may falter ; and the favourable conditions, organic and environmental, may not be forthcoming. The study of these disturbances is a many-sided topic which by no means wholly falls within the province of psychology—the philosopher, the sociologist, the philologist, the physiologist are all in one way or another involved—though the psychologist must always be interested in their results. His own special province is the study of the personal and emotional factors that influence thinking. Thinking is a personal activity which can never be detached from the total personality of the thinker. Detachment is at best a relative term, which in itself can be psychologically defined. This involvement of the personality in cognitive activities has recently been shown in a number of investigations to apply to the activity of perception. It is even more true of more complex conceptual activity.

It has, of course, long been known that personal and emotional factors influence thinking. Did not Plato speak of these matters centuries ago ? What the psychologist has done is to show the unexpectedness of many of the emotional influences to which thinking is subject, and the subtlety of their influence. I shall confine myself to experimental material of recent origin and wholly pass over the more disputable clinical and psychoanalytic evidence, though to say the least I think we could describe this as suggestive. Laboratory experiments in comparison are pedestrian and artificial things, and but a poor reflection of the universe of thought—perhaps, nevertheless, they are a beginning towards a more precise understanding of these high matters. The psychologist can add little as yet to the empirical advice which Graham Wallas, for instance, proffered in his *Art of Thought*; he certainly cannot produce to order ' The Moment of Vision,' of which Sir Kenneth Clark spoke in this year's Romanes Lecture. Yet in the background the humdrum experiments of the psychologist may possibly be laying foundations.

Two groups of students are given a complex sorting task. They are asked to sort 26 cards upon which various designs are pasted into four consistent and exclusive categories, in the way for example that playing cards can be sorted into four suits. The designs however, in this case are complex, and the problem of discovering four consistent and exclusive categories is not easy. The students are divided into two groups ; the first group are confronted with the problem as a test situation ; the second group are given the task of helping a fictitious individual John Jones, who has been given the problem as a selection test for entry to a job : what help can they give John Jones in solving it ? Now this, as I have pointed out, is an artificial situation. Nothing much is at stake for either group. Yet the John Jones group does significantly better. They work at a higher rate, and they produce more correct solutions. They are able in a certain sense to stand back from the problem ; they are more personally detached ; which seems to show that even quite slight conditions of emotional involvement—no need for an emotional upset—may influence thinking (Marks & Ramond, 1951).

Equally influential is subject-matter. Experiments have been carried out in which answers have to be given to the kind of exercises in logic which were appended to the older textbooks of logic. More correct answers are obtained when neutral terms or abstract symbols are employed than with emotionally-toned terms, even though formally the problems may be identical (Morgan & Morton, 1944). It is much harder to draw the correct conclusions from syllogisms about communists, even in the context of an academic exercise, than from syllogisms about Socrates, to many modern students a remote and hardly known figure about whom they ' couldn't care less.'

A rather different kind of enquiry concerns the influence on thinking first of the tools or symbol systems, and secondly of the materials, or information, available. I have no time to dwell on these topics,

though both are important, and some promising experimental approaches have recently been opened up (Oléron, 1951, 1953 : Whitfield, 1951 : Hovland, 1952). I must however, briefly mention some recent experiments on the thinking that is carried out in groups—by committees, for example, which more and more are managing and deciding the course of society. It is important that we should observe, and record, and analyse the decision making of committees, as John Cohen, of Manchester, has done in his UNESCO study (Cohen, 1952). Experimental studies can perhaps help us still further. How far, for example, does size of group affect the capacity of a team to solve problems of the familiar ' Twenty Questions ' type?—a parlour game, but not without psychological interest. At this type of problem experiment has shown that groups are superior to individuals, but large groups are not superior to small groups. In fact a team of two seems to do the job as effectively as any. The main reason for the poorer performance of the single individual is that he (or she) establishes an incorrect set which persists even in the face of incompatible answers. The interplay of even two individuals is enough to dislodge the blockage (Taylor & Faust, 1952). More elaborate experiments have been designed to test the effects of communication patterns on group problem solving. Is free two-way communication between all the members of the group the best arrangement? Or is it best to have a co-ordinator? To some extent the answer appears to depend on the kind of problem confronting the group. In the less structured problems the presence of a co-ordinator who alone has access to all the information appears the most efficient arrangement (Heise & Miller, 1951 : Leavitt, 1951). It seems not unlikely that in this domain the developing theory of communication may point to problems and help in providing answers.

The accomplishments of psychology in this field are still slight ; the field itself coextensive with human life. My examples have been intended to illustrate a method, which may in the course of time, place our intuitive art of thought, which is all we as yet possess, upon firmer foundations, and, in doing so, provide data of theoretical significance.

IV. *The Process of Thinking*

Let me turn to my third and final main topic. It is not enough for a psychology of thinking to provide a genetic account of thinking, nor to explore the conditions affecting thinking. It must attempt, however provisionally, to give some account of the mechanisms or processes involved and of the structure of the intellect. The classical speculative attempts of the philosophers fell broadly speaking into two sets of schools ; the schools which postulated an ordering principle or reason, transcending experience ; and the schools which stressed the associative linkages built up within experience itself. While Kant, again to put it rather crudely, attempted to knit together the two points of view in a new critical philosophy. The history of experimental psychology can perhaps be described as a re-enactment of this divergence and of this attempted Kantian reunion. Connectionist and Gestalt psychologists have formed the two diverging schools ; while Piaget, Hebb and Harlow, with their equal stress on experience and on organisation, are the reconcilers.

I would have placed Spearman in this category had not his attempt in this respect so obviously failed. Spearman's great erudition enabled him to appreciate the strong points of the rationalist schools ; while his scientific bent turned him towards empiricism. The two strands, however, never properly united. Rationalism gave birth to the theory of ' G ' ; empiricism to the noegenetic laws. But there is nowhere a higher synthesis. The factor theory and the noegenetic theory remain separate theories without any organic inter-relationship. Spearman, of course, has a secure claim to fame as the founder of one of the largest psychological industries, factor analysis. While factor analysis, however, has proved its worth in the economy of mental testing, it is much more doubtful whether, as Spearman hoped, it can throw light on the structure of the mind. Factors according to Vernon (1950) are only ' categories for classifying mental performances.' It is perhaps not surprising, therefore, that recent analyses

of intellectual functions, such as those of Meili (1946), Halstead (1951), Rimoldi (1951) and Moursey (1952) show substantial divergences. As to Spearman's noegenetic principles they can only be described as jejeune ; the reduction of all cognitive acts to relation-educing is to simplify the facts of structure and organisation to vanishing point. Spearman's theories have been immensely influential in the field of mental testing, and have provided one of its main theoretical foundations. The weakness of these theories and their lack of integration is perhaps one reason for the difficulties into which the mental testing movement has run.

Thinking, as Sir Frederic Bartlett (1950) has pointed out, is most significantly conceived as a high-level skill : derived and built up, therefore, not, as in older views, from special cognitive elements, but compacted, as all skills must be, of both sensory and motor components. Now skills are essentially activities, and skills are learned. And they comprise, as Bartlett put it, ' a sequence of moves towards some total achievement which is regarded as their completion.' If thinking is a kind of skill or group of skills, then the psychologist can approach thinking, and perhaps hope to elucidate some of its problems, with the concepts derived from the study of simpler skills ; he can bring to bear upon the psychology of thinking all that he has learned about learning. Such a viewpoint is not wholly speculative. It conforms to Piaget's genetic findings. It conforms to the results of perceptual studies indicating the active, not merely passive, nature of perception, and the acquired character of all but the simplest perceptual structures. It conforms to the evidence pointing to the motor accompaniments of thought. Of course, in saying that thinking is a form of skill we do not solve all the problems, because there is a great deal about the nature of skill we do not understand. But we bring thinking within range of conceivable investigation, which a purely ghostly activity was not. Thinking still presents special problems of its own ; for no theory of thinking can dismiss the novel representative factor—that partial interiorisation of activity, the emergence of which can be dated in the human child to the second year of life, and the development of which becomes man's most characteristic mark. We can term these interiorised activities ' operations ' with Piaget, or ' mediation processes ' with Osgood (1953), but their nature remains far from clear.

Now a basic feature of all skills is organisation. In so far as it is a skill, thinking is best conceived not in terms of linkages and relations, but in terms of organisations, structures, patterns or schemata. Only so can the important features of relevance, significance, productiveness be explained. This conclusion was a major outcome of the work of Selz, whose two works written about the time of World War I, Humphrey has conveniently summarised (Selz, 1913, 1922), and of Lindworsky (1923), another worker now largely forgotten in this country. Emphasis on this aspect of thinking is, of course, also the predominant note of Gestalt psychologists, and in particular of Wertheimer, whose book *Productive Thinking* (1945), is one of the most original and suggestive recent contributions to the subject. Wertheimer was himself a genuine thinker, and there is a first-hand freshness and simplicity about his observations of the thinking of schoolchildren and office girls at one extreme, and of Einstein, whom he cross-questioned, at the other. It is the grasp of the inner structural nature of the data that matters in thinking ; the capacity to see the essential pattern of the whole and not be misled by details or stuck in partial aspects. In novel problem situations where the structure is not immediately apparent a key feature of productive thinking is flexibility, an ability to restructure the material and to ignore habitual solutions which fail to fit. This restructuring is not a matter of blind trial and error ; there is in creative thinking a premonitory outline which directs the search ; in vulgar terminology a ' hunch '—in the technical jargon of Selz ' a schematically anticipated complex.' In other words thinking proceeds by means of hypotheses and expectations. This emphasis on pattern and organisation seems all the more acceptable in that it leads on naturally and without undue violence to the formal characteristics of logic, and to those

properties of aesthetic form which certainly play a part in creative thinking both in the arts and the sciences.

The further contention of the Gestalt psychologists that this grasp of structure is an immediate, unanalysable act, most adequately described as 'insight,' is much less acceptable. It ignores the part played by learning, the gradual building up of schemata, the assimilation of experience, for all which there is factual support. The evidence for insight described by Köhler in *The Mentality of Apes* is not conclusive. It was obtained on animals whose past histories were not fully known. A number of investigators have more recently been able to study the behaviour of chimpanzees with known previous experience when confronted with stick problems of the kind used by Köhler. No one has yet reported sudden insight in apes having no previous opportunity to use sticks (Birch, 1945). The place of experience in the ape's ability to solve problems has been confirmed by the detailed observations of Dr. and Mrs. Hayes on the chimpanzee Viki, (Hayes and Hayes, 1952) which they adopted a few days after birth and reared in the home for four years with most of the treatment of a child—a practice incidentally which is becoming so fashionable among psychologists that it may be necessary for a psychologist in choosing a life partner to consider her willingness to undertake such an adoption.

In a series of exceedingly interesting, and I think important, experiments with monkeys Harlow has traced the development of apparently insightful behaviour through the establishment of what he terms 'learning sets' (Harlow, 1949). 'The learning of primary importance to the primates at least,' states Harlow, 'is the formation of learning sets. It is the learning how to learn efficiently in the situations the animal frequently encounters. This learning to learn transforms the organism from a creature that adapts to a changing environment by trial and error to one that adapts by seeming hypothesis and insight.' In one set of experiments a group of monkeys was given a series of 344 discrimination problems. The monkeys progressively improved their ability to learn discrimination problems of a common type. In the early trials of the series the performance was not significantly better than chance; in the last 56 trials the performance of the monkeys was nearly perfect. 'These data,' claims Harlow, 'clearly show that animals can gradually learn insight.'

It would seem, therefore, that thinking is explicable in terms of learning; that insight and organisation are end products, the result of a mature skill gradually acquired by experience and marked increasingly by flexibility, by transfer and generalisation, and by accurate discrimination—processes which logically culminate in abstract conceptual systems of high generality. The central problem would seem to be what is generalisation, and how does generalisation take place? How, in other words are similarities of structure recognised in spite of diversities of context and of internal arrangement? At least this poses what is perhaps the main problem of the processes of thinking in an answerable form, even if we have not yet got the answers. For the processes of generalisation and discrimination are processes which we can study empirically and indeed which can up to a point be copied by means of mechanical analogues. We are not left, as the Gestalt psychologists left us, with a *deus ex machina* in form of insight.

There seems little doubt that flexibility, the capacity to shift from one viewpoint or method to another, and to create new outcomes by grasping previously unrealised structural analogies are among the most important characteristics of intelligence, which tend to be deficient in the subnormal and diminished in pathological states. A number of psychological tests employed in clinical practice have made use of this principle for diagnostic purposes.

The absence of flexibility, or rigidity as it is commonly termed, is unlikely to be of unitary origin. It would seem partly to be a function of low intelligence (Werner, 1946); partly to be a general trait of personality and bound up with a number of other traits besides cognitive rigidity (Rokeach, 1948; Cowan & Thompson, 1951); and partly to be a function of the variety and nature of learning situations (Schroder & Roche, 1952; Buss, 1952). Whatever its origin

rigidity, or ' getting into ruts ' is a conspicuous enemy of productive thinking, as many investigators from Duncker (1945) onwards have experimentally demonstrated. It has been shown again and again that the fixation of habitual methods of solving arithmetical problems or of obtaining the answers to anagrams may interfere with the discovery of new methods, when slightly different problems requiring new methods are presented. Previous experience in certain circumstances has a negative effect on performance. It has also been shown that these ' sets ' as they are often termed may be formed and operate unconsciously (Rees & Israel, 1935).

I have time only to describe briefly one experiment in which subjects were shown a set of lantern slides in eight successive stages of focus, commencing with the most blurred and step by step approaching full focus. At each stage they were asked to describe the content of the slides, and their results were afterwards compared with a control group who saw the same slides only in one or other of the later stages of clarity. There was a much greater accuracy of interpretation by the control group dealing with isolated presentations than for the corresponding slides shown in series to the experimental group of subjects. Interpretations based on the inadequate evidence of the earlier blurred versions persisted and hindered reinterpretation (Wyatt & Campbell, 1951). Can we suggest that this simple experiment is a kind of paradigm of the situation often encountered in research, and, illustrates the way in which workers fresh to the problem may sometimes find the answers that have escaped the old hands?

Between the role of experience as a basis for generalisation and the role of experience as a source of rigidity there is an apparent contradiction. This contradiction is a well-known phenomenon in learning situations of all levels of complexity, and experimental work is beginning to throw some light on it (see Gibson, 1940 and Osgood, 1949). So the fact that we meet with it also in thinking tends to confirm the hypothesis that thinking is a skill, comparable in many ways to other learned skills. Nor is this hypothesis really incompatible, as it might at first seem, with the directedness, creativity and formal beauty of our higher thought processes; for these characteristics are also present to some extent in all our skills. The strange and novel feature of thinking is its adaptation, not to present, but to absent reality through the medium of representative symbolism—an adaptation which we refer to as truth. And this is mainly a problem for the philosophers.

V. *Conclusion*

As Hebb pointed out in his stimulating discourse to the Canadian Psychological Association last summer (Hebb, 1953), no theory as yet comes near to comprehending the full range of the phenomena we know as thinking. In this address I have narrowed the field still further and confined myself deliberately to recent empirical investigations in normal psychology. If I have passed over material from psychopathology and psychoanalysis, as well as the introspective analyses of the older philosophical and psychological schools, this is not at all because I would deny their value and importance. In this dim and still unstructured frontier region of research every road of approach must be explored: we cannot dispense with speculation or insist uncompromisingly on neat and rigid methodological schemas before the ground has been properly surveyed. Our search is still for the main outlines of the landscape, for the possible centres of settlement from which our colonisation can spread.

The psychology of thinking has not yet surmounted the initial difficulty of adequately describing its subject matter. Thinking is still often equated with problem solving, and a problem defined as ' a situation that holds up an organism in its effort to reach a goal.' That thinking proper is essentially problem solving and to be distinguished from idle fancy is, of course, an old view. It is, for instance, the standpoint of Dewey in his well-known book *How We Think* (1910). But it is a view which is increasingly difficult to sustain. There is no sharp dividing line between problem solving and fantasy thinking. As Rapaport urges in the introduction to his *Organisation and Pathology of Thought* (1951) the major need is to

encompass in a unitary framework all forms and manifestations of thinking, both normal and pathological, realistic and imaginative. Even normal realistic thinking is not confined to problem solving; it is both regulative and creative. It may regulate conduct, as for instance when the thread of discourse is maintained as discourse proceeds; while in its creative aspect it expresses 'visions' rather than solves problems. It is indeed impossible to keep these aspects altogether apart. Hadamard (1945) has shown that even in mathematical thinking autistic processes are involved. Moreover it would at this stage be quite dogmatic to assert that thinking is motivated in the narrower biological sense by blockages in reaching overt goals. In a remarkable book on the philosophy of symbolism Langer (1942) suggests that the viewpoint of dynamic psychology, which attempts to explain symbolic developments in terms of biological adaptation and needs, is inadequate to explain the prevalence in human history of magic, ritual, myth, art, and other symbolic manifestations, and that human beings possess a basic independent need for symbolic realisation. Perhaps this is what Wertheimer referred to when he wrote: 'To live in a fog, in an unsurveyable manifold of factors and forces that prevent a clear decision as to action, as to the main lines of the situation, is for many people an unbearable state of affairs.' (Wertheimer, 1945). Or as Langer put it 'Symbolisation is both an end and an instrument.' If this is so, the psychology of thinking cannot legitimately confine itself to the instrumental uses of thinking which are involved in problem solving; it must embrace every kind of symbolic manifestation.

The purpose of my address, however, has not been to pursue speculations of this kind, even if current theories of secondary drives have made them somewhat less speculative. I have had humbler aims: to report some contemporary work by Piaget and by various other experimental psychologists; tentatively to suggest the lines which developments seem to be taking in the interpretation of thinking as a high-level skill; and to show that the psychology of thinking is no longer an esoteric study pursued by aetherial methods, but a branch of psychology which can be objectively investigated. As such it joins hands not only with better settled areas of psychology, but with subjects like biology and logic, which lie beyond its own frontiers, and can perhaps begin to claim scientific respectability.

My thanks are due to Dr. W. Mays, lecturer in philosophy in the University of Manchester, for a number of helpful criticisms particularly of the section on Piaget; and also to the staff of the department of psychology, University of Liverpool, who made various critical comments on the first draft of the address.

References

Bartlett (1950): 'Programme for experiments on thinking.' *Q. J. Exp. Psychol.* 2.

Birch (1945): 'The relation of previous experience to insightful problem solving.' *J. Comp. Psychol.* 38.

Buss (1952): 'Some determinants of rigidity in discrimination—reversal learning.' *J. Exp. Psychol.* 44.

Cohen (1952): 'Some working hypotheses and provisional conclusions in the study of committees and conferences.' *Occ. Psychol.* 26.

Collingwood (1939): *An Autobiography.*

Cowan & Thompson (1951): 'Problem solving rigidity and personality structure.' *J. Abn. and Soc. Psychol.* 46.

Duncker (1945): 'On problem-solving.' *Psychol. Mon.* 58. 270.

Gibson (1940): 'A systematic application of the concepts of generalization and differentiation to verbal learning.' *Psychol. Rev.* 47.

Hadamard (1945): *The Psychology of Invention in the Mathematical Field.*

Halstead (1951): 'Biological Intelligence.' *J. Pers.* 20.

Harlow (1949): 'The Formation of Learning Sets.' *Psychol. Rev.* 56.

Hayes & Hayes (1952): 'Imitation in a home-raised chimpanzee.' *J. Comp. Physiol. Psychol.* 45.

Hearnshaw (1954): 'The Psychological Study of Conceptual Thinking.' *B. J. Psychol.* 45.

Hebb (1953): 'On Human Thought.' *Canadian J. Psychol.* 7.

Heise & Miller (1951): 'Problem solving by small groups using various communication nets.' *J. Abn. and Soc. Psychol.* 46.

Hovland (1952): 'A Communication analysis of Concept Learning.' *Psych. Rev.* 59.

Humphrey (1948): *Directed Thinking.*

— (1951): *Thinking: an introduction to its experimental psychology.*

Langer (1942): *Philosophy in a New Key.*

Leavitt (1951): 'Some effects of certain communication patterns on group performance.' *J. Abn. and Soc. Psychol.* 46.

Lindworsky (1923): *Experimental Psychologie* (Eng. tr. 1931).

MARKS & RAMOND (1951) : ' Problem solving as a function of the situation.' *J. Exp. Psychol.* 41.
MEILI (1946) : ' L'analyse de l'Intelligence.' *Arch. Psychol.* 31.
MORGAN & MORTON (1944) : ' The distortion of syllogistic reasoning produced by personal convictions.' *J. Soc. Psychol.* 20.
MOURSEY (1952) : ' The Hierarchical Organization of Cognitive Levels.' *B. J. Stat. Psychol.* 5.
OLÉRON (1951) : ' Penseé conceptuelle et langage.' *L'Année psychologique*, 51.
— (1953) : ' Classement multiple et langage.' *J. Psychologie*, 46.
OSGOOD (1949) : ' The similarity paradox in human learning : a resolution.' *Psychol. Rev.* 56.
— (1953) : *Method and Theory in Experimental Psychology.*
PIAGET Group I
(1924) : *The Language and Thought of the Child.*
(1924) : *Judgement and Reasoning in the Child.*
(1926) : *The child's conception of the world.*
(1927) : *The child's conception of causality.*
(1932) : *The moral judgement of the child.*
PIAGET Group II
(1937) : *La naissance de l'intelligence chez l'enfant* (Eng. tr. 1952).
(1937) : *La construction du réel chez l'enfant.*
(1941) : *La genèse du nombre chez l'enfant.* (Eng. tr. 1952).
(1941) : *Le développement des quantités chez l'enfant.*
(1952) : *Classes, relations et nombres.*
(1945) : *La formation du symbole chez l'enfant.* (Eng. tr. Play, dreams and imitation, 1951).
(1946) : *La développement de la notion de temps chez l'enfant.*
(1946) : *Les notions de mouvement et de vitesse chez l'enfant.*
(1947) : *La Psychologie de l'Intelligence.* (Eng. tr. 1950).
(1948) : *La représentation de l'espace chez l'enfant.*
(1948) : *La géométrie spontanée chez l'enfant.*
(1949–50) : *Introduction à l'epistémologie genétique*, 3 vols.
(1949) : *Traité de logique.*
(1951) : *La genèse de l'idée de hasard chez l'enfant.*
(1952) : ' Autobiography.' *In History of Psychology in Autobiography*, vol. iv.
(1953) : *Logic and Psychology.*
(1953) : ' Structures opérationnelles et cybernétique. *L'Année Psychologique*, 53.
RAPAPORT (1951) : *The Organization and Pathology of Thought.*
REES & ISRAEL (1935) : ' An Investigation of the establishment and operation of mental set. *Psychol. Mon.* 46. 210.
RIMOLDI (1951) : ' The Central Intellective Factor.' *Psychomet.* 16.
ROKEACH (1948) : ' Generalized mental rigidity as a factor in ethnocentrism. *J. Abn. and Soc. Psychol.* 43.
SELZ (1913) : *Ueber die Gesetze des geordneten Denkverlaufs.*
— (1922) : *Zur Psychologie des produktiven Denkens und des Irrtums.* (Summarized in Humphrey (1951.))
SCHRODER & ROTTER (1952) : ' Rigidity as Learned behaviour.' *J. Exp. Psychol.* 44.
TAYLOR & FAUST (1952) : ' Twenty Questions : efficiency in problem solving as a function of size of group.' *J. Exp. Psychol.* 44.
VERNON (1950) : *The structure of abilities.*
WERNER (1946) : ' Abnormal and Subnormal Rigidity.' *J. Abn. and Soc. Psychol.* 41.
WERTHEIMER (1945) : *Productive Thinking.*
WHITFIELD (1951) : ' An experiment in problem solving.' *Q. J. Exp. Psychol.* 3.
WYATT & CAMPBELL (1951) : ' On the liability of stereotype or hypothesis.' *J. Abn. and Soc. Psychol.* 46.

GROWTH AND PRODUCTION

Address by

PROF. W. H. PEARSALL, F.R.S.

PRESIDENT OF SECTION K

THE biologist today is experiencing a complete revolution in techniques which must rapidly alter the basis of his studies. Much as the invention of the microscope changed the whole outlook of biology by imposing new dimensions upon it, so the development of the X-ray techniques, the appearance of the electron microscope and of other advanced forms of microscopy, the development of remarkable new methods of microchemical analysis, the use of isotopic tracers and of micromanometric methods, are all imposing new dimensions upon our experimental approach, upon our methods of thought and upon our basic biological conceptions. For the moment, no doubt, we are all fascinated by the new avenues thus opened up. Their individual complexity will no doubt lead to increased specialisation of research and to concentration upon very limited fields. At the same time, because they *are* dealing with new dimensions, we often do not know what our results mean, and it will doubtless become more and more important to stand back from time to time and ask ourselves where we are going and also how we can integrate the results we are obtaining.

This is perhaps only a facet of the wider problem of whether our subject should tend towards specialisation into various subdivisions, or should remain unified. A parallel problem is that involved in training the coming generations of botanists—should we aim at a wide and varied training or at a narrow and intensive one? Whatever the ultimate answer to these questions may be, there is, I think, no doubt that the plant as the subject of our studies is a living organism and so poses certain unified and fundamental problems. Thus to an earlier generation evolution and phylogeny were almost the only possible bases for biological thinking and integration. The method of study was that of comparative morphology, for which preserved material was often as useful as the living. To generations trained between the wars, however, the experimental approach has become almost inevitable. For this it is necessary to work with living organisms and thus problems associated with the chain of causation must inevitably be related to the growth and development of the individual in the first instance, and at second remove, to the controlling influence of heredity. The fundamental problem of how this influence is exerted can only be answered by an appreciation of the effect of what we call 'genes' on growth and development. Thus it seems to me that for a long time to come our subject must inevitably be strongly orientated about the field of growth and development, which is moreover one of the unique and distinguishing capacities of a living creature.

While there is nothing very revolutionary about this conclusion it does seem to me that it is in danger of being overlooked in the enthusiasm for new techniques. On the chemical side, the elaboration of micro-analytical methods has led to continued emphasis on substances present in very low concentration, micronutrients, antibiotics and growth promoters in particular, and thus perhaps the continued importance of the mass-laws has tended to be overlooked. There is another danger, that undue attention is given to the search for a substance which will produce a given effect on growth, an up-to-date philosopher's stone, it as were. This may focus attention in the wrong place and too little study may be made of the growth process itself. Sinnott has used an interesting analogy in discussing

this danger. The substance used, like the coin we put into an automatic machine, is only an *evocator*. The response it produces is a function of the mechanism into which it is introduced. Thus the same coin will give a different response in a juke-box, a gas-meter and a fruit machine, or, biologically, the evocator may thus provide a different result according to the organisation of the creature or organ we are studying. This organisation is surely the distinctive and important thing, what we mean when we talk about an *organism*. The presence of such an organisation is a fundamental property of life.

One way in which we are now moving to a new appreciation of what it may mean, comes through the great increase of knowledge which has resulted from micromanometry and from the use of isotopic tracers and paper and column chromatography. Two elements resulting from this field are of the greatest importance, the realisation of the common pathways of intermediate metabolism which is fundamental to our conceptions of the dynamic chemical organisation of the cell, and secondly the greatly enhanced knowledge of the composition of cell proteins (as contrasted with storage proteins) and of nucleic acid, both equally fundamental to a knowledge of the chemical structures of the cell organisation. While mention of the composition and structure of proteins suggests an approach to the biological interest in structure as such, the newer developments in protein chemistry have served more and more to emphasise the dynamic state of cell proteins and of their relations with soluble metabolites—the continual interplay of synthesis and degradation which seems to be a necessary interpretation of recent research in this field. It appears that this might well be a fundamental conclusion, because it suggests a way in which the dynamic chemical organisation of the cell makes contact and is integrated with the dynamic structural organisation of the protoplasm.

Under the impact of the new techniques, our ideas of what we mean by chemical and structural organisation are becoming clearer. We can, no doubt, even think of the act of growth, chemically, if too simply, as the autocatalytic growth of something like a protein—itself the template upon which new metabolic units can only be added in certain places and in ordered arrangement. It is thus possible, in long protein chains, or microfibrils, for there to be topochemical properties, reactions localised in certain parts of the protoplasm which would represent the basis of enzyme action and give the possibilities of polarity or of symmetrical development inside the cell.

While one might imagine that such complex systems were often liable to develop instability, nothing in our experience suggests that they do. One has to suppose, therefore, not only that the protein model has an inherent stability—which we must assume to be genetic—but also, I think, that it functions in an internal environment within which it is stable. It was Macallum, very many years ago, who first drew attention to the apparent impermeability of the nucleus under most conditions and to the significance of the fact in heredity; those who have tried to introduce materials into plant growing points will recognise that these also may severely restrict what can be absorbed and such observations may make it easier to understand the stability in development of what seems to be a labile and dynamic process.

If one were to wish to change the pattern of protein development in growing cells one can imagine that it could only easily be done where the effects of an outer chemical environment could be applied with a high intensity, in organisms, for example, like the bacteria, with a high surface-volume ratio, or in organisms where the nuclear material showed a lower degree of organisation than it appears to do in higher plants. It is probably significant, therefore, that the organisms which can be 'trained' to produce new enzymes or to carry out new reactions seem to be found mainly or entirely among bacteria and fungi.

It would be attractive to continue further this type of analysis, by reference to other problems of organisation that are yielding valuable results. But at least two of these, cytogenetical organisation and morphogenesis, are to be represented by symposia at this meeting, so that it may

be more useful to consider problems which interest a wider public, those arising from the growth of the plant as a whole.

This, from the point of view of the botanist, represents the integration of all the plant's activities. For the moment this subject is scientifically unfashionable, but because the green plant is the basis of the production of food and raw material, the subject must soon come again to the front of popular attention.

Public opinion has come to recognise that the growth of world population is fast outstripping the immediate possibilities of food production and of output of raw materials. In our own country, now largely dependent on sea-borne supplies, we have to accept the fact that we are no longer able to meet the demands of society and industry, and the best estimates suggest that we should have to double our production of food and raw materials to do so. In a generally similar manner it has been conservatively estimated that by the year 2,000 the world's demands for food and raw materials will have increased by between 60 per cent to 100 per cent. Since all the sources depend on plants it is clearly appropriate for botanists to consider the scientific problems which may be involved. Is it likely that we can, by conventional methods, increase the output of our existing fields and crops to the level required? If not, how far would it be possible to develop unconventional means of solving this problem, and lastly, in our own case, how will these considerations affect the way in which we use land in our own small and crowded island? It would be impossible in a short time to answer more than a fraction of the questions involved in these problems and all I wish to do is to try and rationalise, if I may use such a hackneyed and horrible word, the problem from the botanical point of view.

It will, I think, be very evident that if we were to start with a bare field after ploughing, the amount of production we should get might depend on three things. The first would be the extent to which the crop plant used would cover the whole of the ground; the second would be the time it would take to reach the state of maximum cover, and the third would be the rate at which the plant was able to carry on photosynthesis. The two main variables are thus the rate of growth and the rate of photosynthesis. Let us now consider these separately.

It would be useful to start with the rate of photosynthesis. Under summer conditions in Britain, the rate of photosynthesis depends largely upon carbon dioxide concentration in the air; light is much in excess, and though it varies with different plants, generally about 20 per cent to 30 per cent of full daylight is adequate to give a nearly maximum rate of photosynthesis. Clearly, unless the carbon dioxide concentration of the air can be increased—a difficult thing to do under field conditions—the daily amount of production by the plants will, other things being equal, depend mainly upon the length of the day and the duration of adequate light, and at the level of solar energy it is not practicable greatly to increase these. Thus, improvements in the rate of production from this point of view would depend upon finding plants which are more efficient in abstracting the limited amount of carbon dioxide from the air, or else upon manuring the air with additional carbon dioxide. The question then becomes, is there any evidence that different crop plants exhibit inherent differences in their capacity for production? We owe to Professor F. G. Gregory and his colleagues methods of growth analysis which should enable us to answer this question. The rate of production is in effect what Gregory called the net assimilation rate (NAR)—the rate of dry-weight increase per unit of leaf area per unit of time. This can be calculated during the growth of crops by estimating changes both in the dry weight and in leaf area. When Heath and Gregory collated results reported by various workers on a number of different species of plants, they found that all of them showed very similar values for net assimilation rate during the vegetative phase. The evidence was obtained from a variety of countries and environments and for this reason the data are, perhaps, not altogether suitable to decide the point, as they, and also more recent inquiries, have shown differences in NAR which might possibly be due to diversity of

environment. However, when Watson compared three cereals sown at Rothamsted on the same date and in the same environment, in spring, very similar results for the net assimilation rate were obtained. More recent measurements have suggested that some plants, like sugar-beet, may have a somewhat higher NAR. Recent work has also shown that there are variations in NAR correlated with leaf age and also with temperature and with light intensity which give variations of the same order of magnitude as those observed in comparisons between different species. These and similar reasons suggest that the observed specific variations in this value are not highly significant as factors in production, in comparison with the effects of environment and other agencies.

Far different is it with other factors controlling production, such as leaf area. The briefest consideration shows that leaf area must rise from zero at germination to a maximum value at maturity. The curve of growth is of the sigmoid type so general in growth studies, and it is very clear that with even a considerable range in NAR the rate of production of dry matter would depend mainly on the rate of production of leaf area. A plant with a high rate of development, like the cereal, would thus be at an advantage in this respect as compared with a root crop like beet or mangold, since the leaf area would be high at a period of high temperature and long light duration. Thus in the very valuable recent survey of the position, derived from these methods of growth analysis, Dr. D. J. Watson has concluded that variations in dry-matter accumulation depend mainly upon these variations in leaf area and only to a slight extent upon the differences in NAR. The crop plant is, however, a highly specialised type of plant and one can see from the growth curves that a high growth rate may also be associated with early senescence. Only at the time of maximum leaf area will the maximum rate of production be reached. It is clear that the success or otherwise of the crop depends to a considerable extent upon how far and for how long the ground is completely covered. If the cover were permanent and complete, variations in NAR would become the factors determining the yield of dry material. In crop plants this condition can rarely obtain, owing to the short period during which maximum cover is reached. It seems, however, that with a permanent natural vegetation cover, a factor of this sort may become effective in production, though no detailed analysis has yet been made of this type of vegetation cover.

The ideal productive system would be a photosynthetic apparatus which entirely covered the ground and was there all the time—or at least was there whenever the light duration was high, e.g. March to October. Moreover, it would be a system capable of intercepting most of the light available. A single leaf—if we accept Brown and Escombe's data for *Helianthus*—will absorb about two-thirds of the light and, since light is normally in excess, a second leaf underneath it will still be efficiently supplied with light. However, only about 11 per cent of the light would be transmitted through two leaves and this would give light enough for useful photosynthesis in a third leaf layer only if the system were in full sunlight. In diffuse skylight the third leaf layer would barely get enough light to compensate for respiration losses. The useful production layer would thus apparently be not less than two leaves thick nor more than three, if the leaves absorbed light like sunflower leaves.

This conclusion, of course, bears directly on the attempt to grow two blades of grass where one grew formerly. It is clear that this could only be done usefully if the leaf cover over the part of the earth's surface were inadequate—effectively less than the requisite number of two or three layers.

In actual fact there are data available to show that the leaf cover often resembles that envisaged above. If one compares the leaf area of a crop with that of the land it grows on, it is possible to calculate what Watson has called the leaf area index—literally the *average* effective number of leaf layers covering the ground. One may note that these layers may be disposed in varied ways—from the more or less vertical arrangement in a young grass layer to the horizontal layering of a sunflower, for example, which are effective in rather different ways if the light is oblique. But the practical point emerges

that the data available for field crops show that the leaf area index is usually between two and three at maturity (in some 75 per cent of the instances available) and only rarely reaches four.

Somewhat similar results are available for natural vegetation. One method of finding this out is to find the average weight of dead tree leaves per unit area in a wood in autumn and from this to estimate the average number of layers of tree leaves. If the canopy is dense enough to prevent the growth of ground flora, this will give the maximum number of productive leaf layers. Again the average seems to be near two to three for oak and beech woods.

A third way of doing this is to use the point sampling method for analysing herbaceous vegetation. The data available here (referring mostly to moorland vegetation) again indicate that normally the leaf cover is between two and three layers thick. It seems then that only *before* this level is reached would it be possible to increase production by increasing the number of leaves, so that normally it is not of much use trying to grow two leaves instead of one. What *is* important is to grow the leaves early and keep them active late.

This is emphasised again when we turn to consider natural vegetation systems which may be considered to be in some sort of equilibrium with their environments. The only data available for natural vegetation covers in this country consist simply of the dry-weight yields from unit areas of semi-natural vegetations free from material disturbances by man or grazing animals. For obvious practical reasons, the data are collected in late summer by cutting off the standing crop and they do not include a considerable amount of material transported to roots or rhizomes. This inherent difficulty, however, applies also to the estimates of crop yields already considered. The first figures of interest relate to the average annual production of woodland, collected by Dr. J. D. Ovington, of the Nature Conservancy, by felling samples of trees in Forestry Commission plantations which had had known treatment. These are grouped for convenience as evergreen, actually coniferous, and deciduous, or hardwood, trees : data being given as kilograms per hectare. The plantations were between twenty and fifty years old. The data for deciduous trees such as oak, beech and alder are obviously underestimates as they do not take into account the heavy annual loss of material (about 700–1,000 kg/HA) or leaf fall in autumn. The leaf fall losses in conifers must be similar although spread out over a longer period. The annual production of deciduous woodland, about 4,000–5,000 kg/HA, is much less than that of a coniferous woodland, 8,000–12,000 kg/HA, and it is clear that an evergreen woodland has an enormous advantage in production, presumably owing to the fact that the leaf cover is evergreen and is therefore ready to operate under any favourable conditions of light and temperature.

As a matter of interest, data were also obtained from the same localities as to the (probable) agricultural production on the same soils. These are impartial estimates by local agricultural advisers, in one case, perhaps, certainly too high, as the Breckland site makes no allowances for the open heath and includes market-gardening areas. Moreover, in all cases, of course, the agricultural soils represent the better half of the local range and the woodland soils usually the poorer type. Even so, these are second-class agricultural soils, the only ones on which forest remains. On this type of soil, the arable yields come to about 4,000 kg/HA, about what is produced by deciduous woodland. The figures thus show that the large amount of extra labour and the manuring put into the agricultural soils does not yield any return in gross production, though doubtless the quality of the products is different. It thus seems to be clear that the main factor in production is to have a permanent or sub-permanent vegetation cover.

My own interest in problems of land-use and nature conservancy leads me to diverge still further at this point, to make the comment that considerable improvements in production in Britain would involve the use of the poorer soils. Partly for this reason I shall not refer to production in other countries or even to the more highly fertile areas of our own, many of which are more productive than those mentioned here. But we can fairly

compare with the arable soils already mentioned, the yields from natural vegetation in Britain, for this is only to be found nowadays on soils in some way unsuitable for agriculture. The highest yields observed under natural conditions almost all came from swamp soils which are permanently water-logged. Thus reed-swamps and reed-grasses give extremely high figures (10,000–12,000 kg/HA) when found on base-rich soils, and even the poorest water-logged soils (under moorland conditions) have a production of the same order (4,000 kg/HA) as that given for the arable soils. The high productivity of swamp soils is, I think, an almost universal feature that is only slowly being recognised. It is presumably associated with the fact that such swamps not only receive and accumulate the washings from fertile soils, but also that there is a greater solubility of certain plant foods under anaerobic conditions. The evidence clearly seems to call for a new approach to the development of swamp agriculture, particularly perhaps in dry climates like those in East and Central Africa. The high production by swamp plants is the more remarkable in a country such as Britain, when it is remembered that their growth starts late, as compared with the plants of dry soils, and finishes early. They certainly seem to be distinguished by a high growth rate and probably also by a high NAR, though the latter point has not been directly assessed.

The last set of data about production from natural vegetation types shows that even acid moorland soils give yields, again about or above 4,000 kg/HA, comparable with those of crop plants on second-grade arable land. On the more fertile natural soils like chalk downs the yields are much higher (7,000–9,000 kg/HA) and this seems to be associated with the early growth of the grasses and probably also with the high availability of nitrogen, which may also be the cause of more continuous production.

There are thus three forms of high-yielding semi-natural vegetation in Britain, coniferous woodland, evergreen grassland and swamp plants, and proper land-use seems to involve skill in deciding which of these forms is most suitable to the area under consideration and which will involve on that area the minimum expenditure of labour or capital. One must confess to a feeling of some surprise at the evidence that our relatively unfertile and untended areas do really produce as much as cultivated land of somewhat better quality. It seems clear also that one key to high production in some of these cases lies in the long duration of exposure of the leaves, in which coniferous trees lead the way.

There are, of course, considerable differences in the nature of the products possible from high-yielding vegetation. The valuable part of a grass will be protein, not cellulose as in a tree. While it is unlikely that we shall easily find a better source of cellulose or of cheap vegetable fuel than coniferous trees, there are many more possibilities about protein production. A large part of the problem of efficient production here is concerned with the method of collection. The use of animal collectors of protein seems to be wasteful in several respects, even if popular in others. The yield is not more than one-tenth or one-twentieth of the original dry weight of plant, and of this only about half is flesh. To avoid this and on account of the high value of the leaf proteins, N. W. Pirie's pioneer work on new methods of extracting them from fresh tissues offers immense possibilities in utilising the yield more efficiently. In as far as it destroys the photosynthetic tissue in an irregular and untidy manner, cropping by animals must be inefficient. The ideal must be a method like taking a high cut in a layer of grass, to leave a photosynthetic layer thick enough to maintain maximum light absorption and to leave the growing tissues below undamaged.

Diverting almost the whole leaf to man's use would be destructive to the productive unit in a grassleaf, though it would be unimportant in cereal grains. Overcutting or overgrazing would certainly affect both production and composition of a grass sward for example, an extreme case being an example mentioned by Godwin: annually cutting the large sedge, *Cladium*, led to its replacement by smaller rushes or grasses, with an annual dry-weight yield of only one-third the size.

It seems clear that there are considerable possibilities of increasing production both

of raw materials and of food in this country if we can improve our forms of land-use by unsuitable vegetation cover, lengthen the duration of crop cover and improve methods of collecting leaf-protein as food.

Something can also be said for the idea of increased genetic selection towards the possibilities of a higher net assimilation rate and increased yields of leaf-protein. The plants required are clearly those which rapidly produce a protein-rich leaf tissue.

Even when these possibilities have been exploited we shall certainly find criticism, real or implied, as to the scale on which biological production is possible. An industrial civilisation will, not unnaturally, compare the yields of agriculture with those of modern industrial plants. Thus Sir Harold Hartley has commented that a factory such as the new Esso installation at Fawley produces some 50 tons per acre per day, while a modern steelworks will yield 15 tons of steel per acre per day. In contrast, a wheat crop would give not much more than 1 ton per acre per *year*. The biologist would certainly have reservations about these comparisons, but there is no question that modern civilisations are choosing the activities which give the higher scales of production and also wealth, with inevitable losses in food production. On the other hand, in the countries without large sources of power, coal, oil, etc., the reverse of this problem is already raising political issues. Peoples in these undeveloped countries are not prepared to accept for all time the more poorly paid role of food-producers for more favoured societies. They are already, in fact, accusing the larger industrial powers of wishing to reserve the highly productive industries for themselves.

If one looks at these problems of biological production from this angle, one must admit that present-day agriculture leaves something to be desired. The conditions under which it is carried out are largely uncontrolled and uncontrollable, so that the process is subject to all the vagaries of weather and disease. The modern chemical engineer works in systems with elaborate automatic controls in which forecasts of production can be made with great precision while the control of the conditions of reaction is very complete. Moreover, fundamentally in agriculture the situation is that only a small fraction of the sun's energy falling on earth is being reaped; rarely much more than 1 per cent during the life of the crop and, taking it over the whole year, often below 0·1 per cent. The questions for the future thus seem to be whether industrial control of photosynthesis and growth will be possible and whether it will be possible to alter the rates of reaction so much that a quite different order of magnitude may be achieved in production, say ten or a hundred times greater than at present.

It is clear that unconventional methods may be required to solve this problem and probable that controlled systems will be necessary. As a productive system the normal agricultural method suffers, as we have already said, not only from the disadvantages of the plant being exposed to uncontrollable and very variable external conditions, but also of working in a medium inadequately supplied with carbon dioxide (and often with too little nitrogen). Moreover, the product is the integration of growth correlations in a highly complex organism. All of these might be removed in a controlled system and by using an autotrophic micro-organism. The obvious one to use at present would be a unicellular alga like *Chlorella*, mainly because the physiology of this type of organism is more completely known than that of others. An organism of this general type approaches the chemist's ideal of molecules in aqueous solution, so that it is possible to supply it with high concentrations of carbon dioxide and of nutrients, without serious difficulty from diffusion resistances. The properties which make these organisms suitable for industrial use are, of course, those which have made possible considerable advances in physiological knowledge. One point of great interest has emerged from this work, which is that at ordinary temperatures intermittent light is normally as effective as continuous light would be if the flashes used are short in comparison with the dark periods. At about 15° C. maximum effect is obtained with flashes of about $\frac{1}{300}$ to $\frac{1}{100}$ second and a dark interval about ten times longer. This type of result is interpreted as meaning that

photosynthesis consists of a rapid photochemical reaction, the product of which is disposed of by a comparatively slow reaction which goes on in the dark. (There is evidence that this type of dual mechanism is widespread in plants.) Of course, in considering industrial production there would be no point in using intermittent light. What could be done would be to use a thick suspension of the organism, arranging for the cells to circulate very rapidly so that one-tenth of them were exposed to light at the surface for a very short period and were then in darkness for about ten times that time. The whole suspension could thus carry out ten times the amount of photosynthesis which a single illuminated layer would do. If one could combine the results of a treatment like this with a source of nitrogen and high carbon dioxide concentration, it is thought that it would be possible to produce rates of photosynthesis up to twenty times greater than would normally be the case. Clearly one difficulty in doing this would be to apply an appropriate method of stirring the medium. The practical difficulty here has been the high cost of trying to do this.

It should be realised that there is at least one other possibility of increasing the rate of production. If the dark reaction is always comparatively slow, as it is at ordinary temperatures in *Chlorella*, we might hope to accelerate it by raising the temperature, since this type of reaction has normally a high temperature coefficient. A rise in temperature of about 20° C. (say to 40° C.) would be necessary in order to make the light and dark reactions equally rapid in *Chlorella*. For the present, this possibility is ruled out because most of the known strains of this alga will not survive life at temperatures above 25° C. The attempt is, however, being made to find other algae, such as the thermophylic algae of hot springs, which can work more effectively at a high temperature. It is, however, not yet known whether the relations between the photochemical and dark reactions in these algae are the same as in *Chlorella*. The final possibility is clearly to find still other algae in which little or no discrepancy exists between the rates of the two main reactions, always assuming that the photochemical reaction is rapid.

Assuming that all possible problems can be solved, various practical difficulties will will have to be faced. These mostly lie in the realm of chemical engineering and I do not propose to consider them here. Such trials as have been made in industrial photosynthesis with *Chlorella* have been carried out on rather small scales and not over sufficiently long periods. They have been, as far as one can judge, remarkable for the high cost of the product. That, no doubt, is something which may be reduced with more experience. The size of the yields is of more interest. It is comparatively easy to get a dry yield of 2,000 kg/HA per year, which compares not unfavourably with the amount of the yield of a cereal such as wheat. Under the best small pilot plant conditions which have yet been reported, yields as high as 40,000 kg/HA per year have been obtained, though not on a very large scale or over sufficiently long durations of time. If it were possible to produce cells on a large-scale production basis at this rate an appreciable advance could be claimed.

Of course, bulk is not the only thing; equally important is the quality of the product. The advantage of using a suitable alga would be that you could control the nature of the product, protein, fat or carbohydrate, for it is known that the material produced can be varied both with the conditions of growth and also with the age of the culture, as well as by using different algae for different products. It is at least conceivable that industrial application of these methods may concentrate on products of high quality, such as antibiotics, vitamins and the like, rather than purely on bulk production. For this reason alone it is fairly certain that *Chlorella* will not be the only organism used, if and when industrial photosynthesis becomes possible. The favoured organism will probably have a high protein content if only because of its value as food. The nitrogen-fixing blue-green organisms seem to be particularly attractive from this point of view, not only because of their high protein yield, but also because they would not require extraneous sources of nitrogen. As to the quality of the materials possible, one may say simply that little difference exists between proteins and other materials made by the algae and

corresponding types of product from higher plants.

It will, of course, be evident that any solutions of these problems which result in prospective food production are only a small part of possible developments. Such solutions would also involve a widening of our traditional ideas on food, which have, perhaps, changed very little since the Bronze Age and may now, at least, be regarded as due for expansion. It has become increasingly probable that the proteins of active tissues may often be of higher quality than those derived from cereals, for example, even ignoring the presence of ancillary food substances, such as vitamins, in the fresh tissues. It seems likely, therefore, that new protein foods developed from fresh vegetable tissues are likely to become important in the future. Most of the original food plants were chosen because they had organs which could be readily stored and this property has become of greater importance as man has become more and more an urban animal. The enormous recent advances in preparing, freezing and canning food materials seem to offer great scope in preserving the valuable proteins of leaves and green tissues generally as food materials, and it seems along these lines that social and technical advances should proceed in order to make use of probable development in the biological field.

It may be asked what are the prospects that future developments will raise the production of food and new materials to a rate equal to that of increasing populations. My own view, based on what I have told you, is that intelligent land-use, more continuous crop cover and other conventional methods, will go a long way towards meeting foreseeable increases in demand. If we added improved methods of collecting and using plant proteins, and improved protein-producing plants, immediate difficulties should be met. As to the possibilities of industrial photosynthesis, it seems that at first the probable high cost may limit it to products of high quality. But it is only about 30 years since we began to study algal cultures as metabolic systems and already in the last few years about a dozen attempts have been made to explore the working of small pilot plants. A generation which has seen the development of the hydrogen bomb from Rutherford's first experiments might well regard such progress as slow. I think that what I have already said shows that the real advances will be made by those who study the physiology and biochemistry of a wide range of micro-organisms. When more is known of these, more rapid progress may, if desired, be possible.

What we are envisaging in these unconventional types of approach to the production problem, whether we use higher plants or algae, is really the development of systems in which the organism acts as an enzyme complex. The simpler the organism is the more easily we can hope to achieve control of its growth and production. We can, of course, use such a system, when it is found, either to produce goods of a chosen quality, depending on the nature of the enzyme system, or to step up the rate at which the process goes on. The rates of reaction will depend on enzyme content and for this additional reason a high protein content will probably be desirable. There will, however, also be problems of enzyme balance. We can see that in photosynthetic organisms the balance between the chlorophyll-induced photochemical reaction and the dark enzyme reactions will be an extremely important property. Doubtless there will be other problems of a similar sort. Clearly we are only at the beginning of this type of development, and if we are really serious in looking for very high rates of production, we should obviously not rule out the possibility of using organisms such as autotrophic bacteria; because of their very high surface-volume ratios these might be expected to make possible still higher rates of reaction. Moreover, in these general microbiological fields two other highly intriguing possibilities are opening up. One of these is presented by our increasing knowledge of the genetics of micro-organisms and of the possibility of controlling their enzyme systems by suitable genetical treatment. The other possibility is that such organisms can sometimes be trained to produce enzyme systems or to carry out reactions which they do not normally do, and the work of Hinshelwood in particular

embodies ideas which may have astonishing repercussions in the field of industrial biochemistry.

Finally, much progress has been made in isolating the active components of cells and their reactions *in vitro*. The association of essential parts of the respiratory and synthetic metabolism with *mitochondria* is one example of this. Chloroplasts also can now be isolated and with them the major part of the photosynthetic mechanism. If we can take apart the metabolism of cells in this way and so come to understand its working more completely, the future possibility of carrying out photosynthesis *in vitro* can at least be envisaged. Turning this possibility into reality may be a job for the next generation.

It will be noted that here again we come back once more to the questions involved in the chemical organisation of the living cell and of its control by chemical and by cytogenetical methods. If the botanist has any other general comment to make on these developments, it must be to emphasise once again the unrealised possibilities for synthesis that lie hidden in those autotrophic organisms which make use of light.

The usual method of science is the intensive one of pushing an inquiry on a narrow front or to great depth. The wider approach I have ventured to use in looking at problems of growth, organisation and production has perhaps one virtue—that it has enabled us to glance also at some of the growing points of our own subject.

THE TEACHER IN THE 20TH CENTURY

Address by

RONALD GOULD

PRESIDENT OF SECTION L

THERE is a fable told by Pestalozzi of two foals, which were as alike as two peas. The first was purchased by a peasant, who treated it harshly and made it perform menial tasks. As a result, it became unwilling and bad tempered. The second was purchased by a wise master who helped the horse to develop all its noble qualities. As a result, it was loved and admired by all.

Perhaps in the twentieth century we lack Pestalozzi's implicit faith in the teacher's transforming power. Perhaps in some vague way we incline more to Sir Fred Clarke's view of the educative power of parents, teachers, friends, churches, newspapers, the B.B.C. and society as a whole. We must not, however, entirely ignore Pestalozzi's message and fall into the error of minimising the influence of teachers. The quality of teachers will always be one of the determinants of the quality of succeeding generations. It is of the utmost importance, therefore, that the teaching profession should be of the highest possible quality.

There are sound reasons why this question of maintaining and improving the quality of teachers demands urgent attention. Scientific and technical developments are taking place so rapidly that they appear to be the beginning of a second industrial revolution. Mechanical power is being harnessed on such a vast scale that there is an increasing demand for technical skill to utilise it. Education must help to meet that demand.

Material progress raises vast new problems. There are still those who seem to think that an abundance of goods in itself produces the good life. This challenges the moral, spiritual and religious basis of life which our fathers accepted without question. The children in our schools are living amidst these confused standards, and they need help in finding a satisfactory basis for life.

The development of world communications, in eliminating distance, may draw us closer together, but can also throw us together in jostling confusion or even into conflict. The risks of catastrophe will increase or decrease according to the quality of men's minds.

We have a Welfare State, in which the young, the old, the sick, the disabled and the unemployed have become the responsibility of us all. Our nation, to its credit, has shown it has a highly developed sense of social obligation, but if economic recession should come, are we sure we are wise and good enough to sustain such an experiment, even to the point of sacrifice? Again, only the quality of the people of this country will decide.

Our democratic way of life has been and is still being challenged. If it is to survive it must be shown to be superior to all others. That depends, as Jean Jacques Rousseau realised, on the ordinary citizen. 'If there were a god-like people,' he wrote, 'their government would have to be a democracy, but only such people can have a democracy.' Perhaps this is an overstatement, but it is true that political democracy works well only when citizens are wise in judgment and willing to work for and to contribute to the common good. Thus for yet another reason the country needs quality of mind and character.

It is unnecessary to illustrate further. Clearly in our rapidly changing world we need a more carefully cultivated race, and teachers of high quality have a considerable contribution to make to that end.

A teacher of high quality has four characteristics; first, technical competence, a knowledge of the subject to be

taught ; second, teaching ability, the power to communicate knowledge ; third, a broad educational background ; and fourth, personality, a combination of personal characteristics, difficult to define but easily recognised, which enable him to stimulate and encourage his pupils individually and collectively.

Let me hasten to add, however, that this does not imply that teachers should all be trained in the same way, or have precisely the same qualities. Different kinds of technical competence are required in the various parts of the Education system. The knowledge required to teach subjects at University degree standard is hardly the same as that required to teach Elementary Arithmetic. Ability to teach infants is not the same as the ability to teach older children. Possibly many of us could teach older pupils with some degree of success, but we should fail completely with young ones. Then, too, people with vastly different personalities and educational backgrounds have become successful teachers. But technical competence, teaching ability, educational background and a suitable personality, varying in kind and degree, are necessary in all teachers.

Most people would think it desirable that teachers should satisfy all these four criteria, but in practice, a number of people, including some in responsible positions, are content with only three. They imagine that provided the teacher of infants or juniors has a suitable personality, can read and write and teach these skills, a wide educational background is unnecessary. I disagree entirely. We need well-educated teachers in all parts of the educational system.

It is not my purpose here to deal with the whole range of inducements which may help to secure recruits to the teaching profession. I propose to discuss three conditions which must be satisfied if we are to improve the quality of the teaching profession. First, teachers must realise the need for continuing their education, and the necessary facilities must be provided ; second, teachers must be assured of considerable freedom in their professional work ; and third, teachers and others engaged in Education, must regard the service as a developing partnership.

Let us look first at the continued education of teachers. Every teacher I have met regards a General Certificate of Education at Ordinary Level in five subjects plus two years in a Training College as inadequate preparation for the exacting task of educating the young. In the short space of two years knowledge may be increased in a few subjects and ability to impart knowledge to others may be developed, but little more is possible. Our colleagues who teach in Training Colleges recognise this and they too are insisting on a longer course.

Towards the end of this decade, when the demand for teachers slackens, because the large number of children born in the immediate post-war years will be reaching the end of their school careers, it should be possible to extend the Training College course to three years. More adequate attention could then be paid to the theory and practice of education. A more broadly based education could be provided, and a clearer understanding given of the contribution education can make in a changing world. Higher academic standards could be achieved, and teachers of greater educational maturity could be produced.

Whitehead reminded us that there are three stages in the growth of the mind to maturity ; the romantic stage, when imagination is fired and curiosity aroused ; the stage of precision, when the facts are mastered ; and the stage of generalisation, when the relationship between the facts is seen, when the mind forms patterns from the facts. This maturity, this wisdom it might be called, is never achieved until the mind has been subjected to the discipline of learning. Such a maturity is needed in all leaders of thought and action, but in none more than in teachers.

To-day the range of knowledge is so wide that none of us can ever hope to do more than glean some of the facts from a few of its fields. The knowledgeable and the wise in one field will often be the ignorant and the stupid in another. But all teachers, I think, should have detailed knowledge of the philosophy and art of Education, of what Plato, Socrates, More, Rousseau, Pestalozzi, Spencer, Adams, Nunn, Clarke and many more have thought about the task of the educator. From this, some knowledge of the world,

and an understanding of what is practicable in school, they can distil their own wisdom.

Any who have knowledge of classroom practice will realise how important this is. Some teachers have no clear idea of the purposes they should serve, and so they have no means of knowing whether their curriculum and methods are appropriate. They are at the mercy of every passing whim or slaves of mechanical routine.

Uncertainty about the purposes of education was clearly revealed at the end of World War II. Authoritarianism was so much in disrepute that children were sometimes given too much freedom and responsibility for their immature minds. Some schools experimented in democratic control, oblivious of the fact that the responsibility for the education of children does not and could not rest on the children alone, and that democratic control in any real sense is thus impossible. Some interpreted freedom to mean that children could learn without tears, dispense with the hard graft of study, and with all the discipline of learning.

All this shows what errors are made when teachers lack a clear sense of direction and fail to distinguish between what is and is not practicable. It may ease our feelings to blame the theorists who advocate such practices, or the teachers who reluctantly or enthusiastically accept them, but this provides no safeguards for the future. There is but one real safeguard. We can make progress in education and avoid foolish experiments if teachers neither regard every established school practice as sacrosanct, nor condemn every new idea, but trust their own judgment, basing that judgment on a clear conception of the purpose of the education to be provided, and on what is practicable in the classroom.

Much more than a three-year training college course is needed to ensure this. Every serving teacher should look to his own education, working out his own philosophy and striving to achieve that maturity of mind which will enable him, not only to sift the wheat from the chaff in current educational thinking and practice, but to grapple successfully with other problems I shall mention later.

How can this be done? Serving teachers already run many Refresher Courses, thus showing they are conscious of the need for further education. Still more courses are required, and no one should be deterred if only small numbers can attend. Many small local courses have proved most profitable. The Institutes of Education and the Ministry of Education are also providing courses for teachers. Their value is enhanced when they are deliberately planned to meet the expressed needs of teachers. Exchanges with American and Dominion teachers have proved a most valuable educational stimulus to those who have taken part. These schemes should be extended. I suggest, too, that experienced teachers should be released from school for six months or a year on full pay for the purpose of undertaking some worthwhile educational investigation. Above all, every teacher should read widely and keep abreast of modern thought, for no matter what facilities are provided, the teachers themselves will largely determine their own quality. But since this question is fundamental to the future of education, all concerned might consider what more might be done.

Now let me turn to the teacher's freedom. I have listened to many speeches and even more perorations on this subject, and have frequently been left bewildered. I have heard indignant protests that the teacher's freedom has recently been curtailed, but it has been difficult to understand precisely in what way. I have heard flamboyant claims that schools in this country are more free than elsewhere, or that teachers in private schools are more free than those in publicly maintained schools, and the claims have been vague and unconvincing.

During the war, Roosevelt did a great service to democracy by defining more closely the term 'freedom.' He rallied all the free world by declaring that we were struggling not for freedom in general, but for four specific freedoms, freedom of speech, freedom of religion, freedom from want and freedom from fear. Our war aims then took on a new and a deeper meaning, and our objectives became clearer. Similarly, in education, freedom is given reality when more closely defined.

The teachers are concerned with four essential freedoms. The first is freedom

from the fear of arbitrary dismissal. Dismissal for all sorts of capricious reasons was quite common until recently, but through hard struggles, teachers have established this freedom. Legally it is still possible to dismiss a teacher, subject to the usual notice, without assigning any reason whatsoever, but the Local Authorities have voluntarily surrendered their legal rights in this respect to give the teachers greater security and greater liberty. They negotiated with the teachers' conditions of tenure, giving every teacher threatened with dismissal the right of appeal, and thus arbitrary dismissal is virtually impossible. This newly established freedom, however, is not shared by teachers in Private Schools. Indeed, if their security of tenure has improved, it is largely due to the improvements won by teachers in Maintained Schools.

My second is Civic freedom, the right of a teacher to assume all the responsibilities of a citizen, provided, of course, that the education of the children does not suffer, and that he makes no attempt to introduce party political propaganda or sectarianism into his school. This, too, is not a freedom established by law. It has been conceded by Education Authorities because of constant pressure from teachers. Teachers in Private Schools and Public Schools do not always share this freedom. Indeed, I recall a fairly recent case of a Public School teacher who was dismissed from his post for daring to run as a candidate in a local election. Even in the Maintained Schools this freedom is not wholly secure. In one county a teacher is still denied promotion if he dares to hold certain political views. But to me civic freedom means the right to take part in any political activities not legally proscribed, and even in those political activities with which I fundamentally disagree. This then is a freedom won through long struggles, but which is still threatened.

My third is freedom from compulsory duties not connected with the work of a school. Only a short time ago, teachers were sometimes appointed to County Schools, not on the basis of their qualifications as teachers, but because they were prepared to be Choir-masters, Sunday-School Superintendents or Parish Clerks, or to assume some other responsibility in the district. Happily these days are gone, largely because teachers have insisted that appointments must be based on professional merit, and not on willingness to perform duties extraneous to the work of the school. This, too, is a recently established freedom, but one that needs to be guarded with care, for the word ' voluntary ' is used in such subtly varied ways, and so many pressures can be brought to bear, that occasionally this freedom is denied even to-day.

My fourth, and last, is academic freedom. On this much has been written and spoken. We are often reminded of Dr. Arnold's reply to the Colonial Governor who sought his help in selecting a headmaster :

> ' Nothing can be more proper,' he wrote, ' than that the Headmaster or Principal of the proposed school should be subject to the control of the Governor, or of the Bishop if there be one in the colony. I am only anxious to understand clearly whether he is to be in any degree under the control of any local Board, whether lay or clerical ; because if he were, I could not conscientiously recommend him to undertake an offer which I am sure he would shortly find himself obliged to abandon. Uniform experience shows, I think, so clearly the mischief of subjecting schools to the ignorance and party feelings of persons wholly unacquainted with the theory and practice of education, that I feel it absolutely necessary to understand fully the intentions of the Government on the question.'

Thus Arnold claimed the teacher must be completely independent in the actual working of the school. His biographer states he took his post at Rugby only on this condition. Any attempts at outside control he resisted ' as a duty not only to himself but to the master of every foundation school in England.'

In practice, however, no headmaster can be completely independent. Even Arnold had to work within the limits imposed by law, and the opinions of the parents of his pupils could not be ignored. He neither acted illegally nor flouted the parents. There are similar limits to academic freedom in publicly provided schools. The freedom of teachers is limited because children are compelled to attend school, and Governments and Local Authorities must be assured that reasonable education is provided for them. Public Opinion, too, would make itself

felt if teachers appeared to be acting unreasonably.

Whitehead realised there were limits to the teacher's academic freedom, for in his *Adventures of Ideas*, he wrote :

' The State represents the general wisdom of the community derived from an experience wider than the topics of the various sciences. The role of the State is a general judgment on the activity of the various organisations. . . . But where the State ceases to exercise any legitimate authority is when it presumes to decide upon questions within the purview of sciences or professions. . . . The general community is very incompetent to determine either the subject matter to be taught or the permissible divergencies to be allowed, or the individual competence. There can be only one effect, and this is to general professional opinion as exhibited in the practice of accredited institutions. . . . The State of Tennessee did not err in upholding the principle that there are limits to the freedom of teaching in schools and colleges. But it exhibited a gross ignorance of its proper functions when it defied a professional opinion which throughout the world is practically unanimous.'

Thus Whitehead does not object to the State exercising a general control, and probably he would have agreed that Public Opinion imposes limitations. He actually inferred there was another limitation, that imposed by the consensus of professional opinion. No one would support a teacher who spent all his time teaching singing, or who, because of some whim, ceased teaching English, for such libertarianism flouts the general body of professional opinion.

But within the limits imposed by law, public opinion and the broad consensus of professional opinion, the teacher should be free to choose both the content of his curriculum and the methods to be used. This, now generally accepted, is a comparatively recent freedom in the publicly maintained school. Content and method were rigidly controlled, but a charter of liberty appeared in 1905 in Morant's Introduction to *Suggestions for the Consideration of Teachers* :

' The only uniformity of practice,' he wrote, ' that the Board of Education desire to see in the teaching of public elementary schools is that each teacher shall think for himself and work out for himself such methods of teaching as may use his powers to the best advantage and be best suited to the particular needs and conditions of the school. Uniformity in details of practice (except in mere routine of school management) is not desirable, even if it were attainable. But freedom implies a corresponding responsibility in its use.'

Academic freedom is the most important freedom of all to the teacher. It permits him to teach what he believes to be best, and conviction adds effectiveness to his teaching. Without this freedom he would carry little responsibility ; and consequently could not claim to belong to a profession. But with it, acting within the limitations I have mentioned, he can exercise his judgment as to what is best for his pupils, thus making his work truly professional. It follows that only those within the profession should be regarded as competent to evaluate his work.

All these four freedoms have been acquired in the last fifty years. The good old days when teachers were free never existed. I do not doubt that egalitarianism has recently robbed some schools of some minor freedoms, but the four major freedoms have been more firmly established to the advantage of teachers not only in the Maintained Schools, but in the Independent Schools as well.

The price to be paid for freedom, however, is not only eternal vigilance, but the acceptance of responsibility. Happily there is no frontal attack on the teacher's freedoms, but there are still insidious attempts at encroachment. There are at least three potential dangers. The first comes from the planners, who are so obsessed with planning that they wish to plan everything both locally and nationally. Some have already attempted to interfere in content and method, but this interference has been and should be resisted. Then, too, agreed syllabuses in Religious Instruction are compiled by joint committees representative of Local Authorities, teachers and churches. No doubt these are superior to those previously compiled by Local Authorities. No doubt they are flexible, and it is possible to form one's own syllabus from them, but, let us face it, they are based upon the assumption that the teacher cannot be trusted to determine content and method in religious instruction, and so encroach on academic freedom. I

hope that in time we shall obtain for the teacher the same academic liberty in Religious Instruction as he has in other subjects.

The second danger comes from that confusion of mind which interprets equality as uniformity. In some localities to-day strenuous efforts are being made to integrate the various departments of Local Government. Possibly John Stuart Mill, who believed in one elected authority for dealing with all local business and was a pioneer in unifying Local Government, would have applauded these attempts. However, I am apprehensive about some of the consequences. The idea, strongly held by some, that all Local Government employees should be treated in precisely the same way and be subject to the same controls, works to the disadvantage of the education service. I would not say that teachers are either better or worse than other Local Government employees, but I do say that they are professional people working in a different sphere, that the nature of their work is different and their conditions of service are different, and the cult of uniformity threatens to encroach on the freedom they now enjoy.

The third and the most serious of the dangers is that some teachers may be reluctant to accept the responsibility that freedom entails. There are many who chant the slogans of freedom, whilst wearing chains that they themselves have forged. Some do not want freedom. They fear it. They prefer the safety of mental slavery to the risks of freedom. It is nonsense to talk of freedom to choose both content and method, if teachers accept without question the advice and opinions of others. Only teachers of high quality, only those willing to accept responsibilities, are worthy of freedom. They alone can maintain and extend freedom in the teaching profession.

I have now examined how the education of teachers can be improved and how their freedom can be maintained. I must now deal with the third condition to be satisfied if we are to achieve real quality in the teaching profession, which is that all concerned with the Education System, teachers, Local Authorities, the Government and parents, should regard themselves as a partnership. Just as teachers need an area of freedom within which to work and within which there is no interference, so similar conditions must be established for each of the partners, but to this must be added understanding, trust and goodwill, all incalculable, but all of the utmost importance.

Sir Graham Balfour, a former Director of Education for Staffordshire, defined the function of Educational administrators very clearly in 1931, when he said :

> ' Educational administration exists to enable the right pupils to receive the right education from the right teachers, at a cost within the means of the State, under conditions which will enable the children best to profit by their training.'

To him administrators and teachers were the servants of the child. As such a conception became more generally held, what could be more natural than that the servants of the child should regard themselves as partners ? Of course misunderstandings and conflicts occur, sometimes on the speed of advance, sometimes on the means adopted, but, because we are conscious we all serve the child, the partnership is real.

This, however, is a recent conception. Mr. H. C. Dent has told us that in the eighteenth century there were three basic ideas about the provision of popular education. It was a charity, a gift conferred by the well-to-do on the poor ; it was a voluntary and not a State effort ; and it was under ecclesiastical auspices. Since charitable and voluntary organisations almost inevitably depended upon the use of cheap labour, the salaries and status of teachers were deplorably low. Moreover, clerical control greatly curtailed the teacher's liberty. Diocesan bishops licensed and appointed all teachers, prescribed curricula, shaped timetables, organised the internal arrangements in schools, and until 1840 even controlled the appointment of H.M.I.'s. Such powers made a partnership with teachers impossible.

There was little change after the establishment of school boards in 1870. They engaged in sectarian controversy, sometimes involving the teachers. Board members paid frequent visits to schools, checking and signing registers, spending

much time trying to improve the attendance figures to earn a larger grant. Teachers were hemmed in by numerous regulations, and there was much red-tape. So little appreciation of the teacher's task often provoked open hostility.

Nor were teachers' relationships with H.M. Inspectors any better.

In 1875, Mr. Highton, a local secretary of the National Union of Elementary Teachers, received this letter from an H.M.I. :

' Mr. Highton,

'I have not received any instruction from the Committee of Council on Education to hold official communication with Associations of Teachers.'

In 1888, Mr. W. J. Pope, National President of the same organisation, made a violent attack on the Inspectorate :

' When a teacher has finished his course of training,' he said, ' why should his certificate be annually endorsed by any irresponsible person ? The physician and lawyer are not thus held absolutely in the power of any one man, and why should teachers be ? Even the keepers of beer-shops do not have their licences endorsed until they have done something wrong, and not even then until that wrong has been proved in open Court. Why should teachers be kept through this thumb-screw in abject servility to an Inspector ? '

Relationships between teachers and parents were not good. The ' School Board Man ' was hated, for he enforced attendance at school. Absenteeism was rife, and many parents were hostile rather than co-operative.

After 1902, when the Education Service was handed over to County Authorities and to the larger non-county boroughs and urban districts, relationships between the teachers and H.M. Inspectors, and between teachers and Local Authorities slowly improved. The bad effects on relationships of payment by results were wearing off, and in 1905 the new *Suggestions for Teachers* urged teachers to use their newly acquired liberty.

Relationships between teachers and the Board of Education were certainly strained in the time of Morant, but in 1911 Selby-Bigge endeavoured to establish a partnership. He was later assisted by Mr. H. A. L. Fisher, whose Education Act is now somewhat derided, but whose work in establishing a real partnership in education has never been sufficiently appreciated. His contribution was magnificent. He established the Burnham Committees, which not only enabled teachers to settle their salary problems nationally, so easing local relationships, but accustomed Local Authorities and teachers to the idea of working together. Teachers now expect consultation on an equal footing on all kinds of problems at both local and national levels. He established the Secondary School Examinations Council, the Adult Education Committee, and the University Grants Committee, all of which, in different spheres, established partnerships in the Education Service. He adopted the Kempe principle of paying to each Local Education Authority an annual percentage grant related to expenditure. A block grant would have weakened or possibly destroyed the partnership. Sharing the financial responsibility in such a way that every development involved both Government and Local Education Authorities, strengthened the idea of partnership. As a result of all this, the relationships between Authorities, Governments and teachers were greatly improved.

Unfortunately, however, the relationships between clergy and teachers, which had never been good, deteriorated because Local Authorities tended to use Clerical Inspectors to inspect Religious Instruction. Inspection of the work of one profession by members of another was detested by teachers, and this practice bedevilled relationships for many years.

In 1944 came the Education Act, by which more power was given to the Minister, and the ultimate responsibility locally was given to County Councils and County Borough Councils. Divisional Executives, Managers and Governors were also given a place in administration, though their powers were loosely defined. The system is complicated, vague, indefinite, and whilst perhaps understood by Englishmen, is totally incomprehensible to foreigners. Yet it has the supreme merit of satisfying Bentham's test which was not ' Is it logical ? ' or ' Is it tidy ? ' but ' Does it work ? ' It certainly does, but only a politically mature nation could make it work.

Thus the administrative power in education is divided amongst the Minister, 146 Local Authorities, hundreds of Divisional Executives, thousands of managerial and governing bodies and quarter of a million teachers. Such a dispersal of power is politically sound. To establish any Dictatorship it has always been necessary to seize control of the schools and the teachers, and de-centralised control makes the operation very difficult. I have heard it said that the existence in this country of 146 strong, vigorous Local Education Authorities safeguards democracy and lessons the risks of dictatorship. No doubt this is true, but an even greater safeguard is the existence of a quarter of a million teachers who are free to decide what should be taught and how it should be taught.

To-day there is no threatened dictatorship in education in this country. On the contrary, successive Ministers of Education, to their credit, have willingly shared with others powers legally their own. The establishment of many Advisory Committees by the Minister is evidence of the voluntary sharing of power and of a desire for a real partnership. The recent report of the Manpower Committee showed that the present Minister, whilst retaining six key points of control, had willingly yielded to the Authorities other powers which are legally hers.

Similarly, individual Authorities have yielded some of their powers. They could have established what Sick Pay and Tenure regulations for teachers they pleased, but they consulted teachers on a national basis. The recommendations of the Joint Committee were sent to all Local Authorities, and nearly all Local Authorities have adopted them. Some Local Authorities have also devolved other powers. Hertfordshire, for example, trusts the head-teachers with the expenditure of public monies to an extent that would have been deemed incredible a few years ago.

Indeed harmonious relationships are invariably based on trust, on understanding, and sometimes on willingness to yield powers and to forgo rights in the interest of the whole service. The enemies of partnership are distrust and lust for power. Happily, at the Ministry of Education civil servants regard administration as teachers do teaching, as a matter of personal relationships transcending mere machinery. Between the territorial officers of the Ministry, for example, and the Chief Education Officers, there are such close personal relationships, even friendships, that they are important factors in effective working. In the same way, the personal relationships between officers of Local Authorities and teachers, between teachers' representatives and the Ministry, and indeed between all the partners in the Education Service has been so improved that they now make a major contribution to the efficient working of the service.

Where, however, there is a lack of consultation and personal contact, as there is for example in the matter of Visiting and Part-time teachers' salaries, friction and mistrust follow. The spirit of partnership can only be preserved by constant effort. Unfortunately, there are still parts of the educational system where bad feelings exist, but more consultation would do much to oil the wheels and establish harmonious relationships.

In certain other relationships, too, marked improvements can be seen. Parent-teacher co-operation has never been so effective, and now that Clerical inspection of Religious Instruction in County Schools has been abolished, the old relationship between the clergyman and the teacher of Inspector and Inspected has given place to a new and happier relationship in which both clergy and teacher are recognised as working towards similar ends but in different ways.

I have now looked back and shown how partnership has developed, but when I peer into the future I see dangerous possibilities. Perhaps Local Government will be reformed. If so, it is of the utmost importance that it should not be so altered as to imperil the freedom of the school and the partnerships that have been created. Any tinkering with machinery which ignores these two elements will do far more harm than good. In 1956 the Education Grant formula is due to be reviewed, and this will determine the balance of financial responsibility between the Local Authorities and the Government. Again there are risks. The balance of financial responsibility must not be

tipped in such a way as to re-distribute power, with possibly serious effects on the freedom of the teacher and the existing partnerships.

And since a system of this kind can only be maintained by people of high quality, I can only hope such people will be forthcoming in sufficient numbers in the future. I have no doubt that we shall be well served by the Chief Officials of the Ministry of Education and of the Local Education Authorities. But I confess I have some doubts as to whether we shall be able to attract sufficient people of the right type as members of Local Education Committees. There are many admirable and useful citizens whose help would be invaluable, and yet they do not offer themselves for election. Progress in the past has been made possible by men like Sir Henry Lunn, Sir James Aitken, Alderman Byng Kenrick and many more. Men and women of similar calibre will be required to face the problems of to-morrow.

In any case, the education system will always be the richer if teachers of high quality are found in our schools, if teachers are given a large measure of freedom, and readily accept its responsibilities, and if they become real partners in a great service designed to serve the best interests of children and society.

AGRICULTURE: THE INDUSTRY AND THE SCIENCE

Address by

DR. R. E. SLADE, M.C.

PRESIDENT OF SECTION M

AGRICULTURE is an ancient craft and an ancient industry. The writings of Cato, Varro, Virgil, Pliny and Columella tell us of the craft as it existed in ancient Rome and of what had been learned from the more ancient civilisations of Greece, Egypt and Babylon.

These Romans had almost all the general knowledge of agriculture which we possessed about a hundred years ago. They used ashes and dung to enrich the soil. They used rotations of crops, and Virgil recommends lupins in the rotation 'as the pulses are the only crops which enrich the land instead of taking something from it.' Pliny tells us that there is no manure more beneficial than a crop of lupins turned in by the plough. Columella recommends the growing of turnips for feeding cattle, and Cato advocates feeding sheep on the land before growing corn. They give instructions for growing lucerne and recommend the use of chalk on this crop. Pliny mentions a method discovered by the provinces of Britain and Gaul of feeding the earth by means of itself with the kind of soil called marl. He says chalk is also used in Britain, its effects last for 80 years, and 'there is no case of anybody having scattered it on the same land twice in his lifetime.' They must have used heavy dressings.

The Romans used iron ploughs. Not only the simple spiked plough used in Egypt, but ploughs with a coulter to cut the sod as well as a share. But they had no mouldboard, so they did not turn the furrow over. They used to plough twice —along and across. The Roman system of farming was based upon the use of slave labour. At this time when the Romans were using and improving the arts of agriculture which had developed from the earlier civilisations, the German barbarians living in Jutland, having learned the secret of working iron, invented a system of tillage which enabled them to cultivate the heavy clay soils of Northern Europe. They made a heavy plough with a coulter, a share and a mouldboard to turn over the sod. This plough was drawn by four, six or eight oxen and with it they were able to cultivate the heavy forest lands which had been unworkable previously. These German barbarians prospered, increased in numbers and flowed over Europe. They took the plough into Britain, where it was probably used for paring and burning. So Britain was exporting corn to Europe before the Roman invasion. The Germanic system of agriculture, based on the use of the heavy plough, resulted in small communities continually encroaching upon neighbouring tribes under pressure from the increasing population at home. These were the Germans that Julius Caesar threw back over the Rhine. Some centuries later they overran Gaul.

In the seventeenth century the Pilgrim Fathers took this same plough to America, prospered, grew rapidly in numbers and in two hundred years had pushed their way across the continent, in much the same way as their ancestors had spread over Europe some two thousand years before.

After the fall of the Roman Empire this Germanic system of agriculture settled down into the manorial system of farming —usually a rotation of three open fields, fallow, wheat or rye, then barley or oats —or the system of paring and burning, which required a plough that would turn

over a shallow cut of turf without mixing it with the subsoil.

The manorial system with the open fields lasted for a thousand years and longer. It began to decay after the Black Death in the fourteenth century. The Renaissance then spread over Europe the knowledge of the more ancient systems of agriculture, and the invention of printing made this knowledge more easily available. Men wanted to try other rotations of crops, and to try new crops ; they also wanted to keep sheep on land then under the three-course rotation. None of these changes could take place so long as the open fields were maintained. The only alternative was to enclose the fields and start a new system of farming.

About 1600, Pliny's Natural History was translated into English and printed. It went through a great many editions during the next hundred years. From about this time many books on agriculture were written. Sir Richard Weston, about 1650, advocated the use of clover and turnips in the rotation—crops that had then become common in Flanders. The growing of arable crops which could be preserved for the feeding of cattle in the winter initiated a great improvement in the farming system. It had been customary to kill all the animals which might not survive the hungry winter, and to preserve the meat in salt. With clover hay and turnips in clamps they could not only keep more cattle through the winter, they could fatten some of them and produce some winter milk. They also fed turnips to sheep. But the introduction of new systems of cropping only came in very slowly. A contemporary writer states that only educated landowners adopted them.

The enclosure of the manors proceeded steadily over the centuries and was almost complete by 1850. There are, however, today many thousands of acres which are not properly farmed because large numbers of commoners have grazing rights over the land. These areas of grass are never fertilised, re-seeded or drained. In Scotland a Commission which has recently examined the problems of crofters in the Scottish Highlands and Islands found that the law and practice of the townships may bear heavily on the holder who seeks to depart from the traditional modes of husbandry, and the Commission refer particularly to the rule that the arable land of the township must be thrown open to the township's stock after the crop has been secured.

The writings of Arthur Young and of William Marshall give us some very definite information of the state of agriculture in this country between 1750 and 1800. The Board of Agriculture was established by Pitt in 1793 and Arthur Young was the first Secretary. The Board obtained and published reports on several of the best-farmed counties. In these most of the land had already been enclosed. Among these was Hertfordshire, in which I am particularly interested. William Ellis of Little Gaddesdon had published *The Practical Farmer or The Hertfordshire Husbandman* in 1732. Walker made a report on the county to the Board of Agriculture in 1749 and Arthur Young came himself in 1804 to draw up a 'General View of the Agriculture of Hertfordshire' for the consideration of the Board. He visited Mr. Parris, of Parsonage Farm, Sawbridgeworth, which adjoins my farm—but has a heavier soil than mine. He writes: 'In the clays and strong loams of Sawbridgeworth we find the undermentioned course of crops :

1. Fallow; 2. Barley; 3. Clover; 4. Wheat;

also

1. Fallow; 2. Barley; 3. Pease; 4. Wheat.

Mr. Parris showed me a field of very fine wheat ; part under the first of these courses and part under the last : both very good : but the stubble was cleanest on the clover part. Whatever fault may be found, and perhaps justly, with so much fallowing it must be admitted that their crops are good.' This last remark might well have been made by an executive officer of a County War Agricultural Committee in our time.

Arthur Young goes on to say: 'Wherever turnips can be sown, we find them ; and consequently fallows are confined, as they ought to be, to the most difficult and impracticable soils.' He also quotes Walker: 'While weeds continue to grow and increase in the best cultivated lands, fallowing will be practised in Hertfordshire and elsewhere, where farming is under-

stood, till a substitute, less expensive, and equally successful in destroying weeds, is discovered.'

Discussing the yield of wheat, Arthur Young states that Mr. Parris gets 35 to 40 bushels per acre after fallow and nearly as much after clover or pease. But his average is 25 bushels.

At this time the farmers of Hertfordshire used fertilisers brought from London by boats on the canalised River Lea and River Stort. These included soot, wood ashes, shoddy, bones and bone waste from the button makers. They top dressed their corn with soot, a supplier of nitrogen. I believe these Hertfordshire farmers introduced the practice of top-dressing farm crops which were backward.

The century from 1800 to 1900 was a difficult one for our industry although the population increased from 11,000,000 to 40,000,000 in a hundred years.

The Napoleonic wars and their aftermath caused a depression in agriculture for 40 years. Then for a period of 30 years agricultural prosperity accompanied the expansion of industry and of exports. But from 1870 we imported more and more food in place of gold in exchange for our exports. The imports of food increased from £51,000,000 in 1850 to £183,000,000 in 1900 and to £219,000,000 in 1910. The chief imports were wheat and meat. The area under corn fell from 8¼ million acres in 1871 to 6 million acres in 1901. The area of permanent pastures increased accordingly. But there was little use for the pastures because from 1875 dead meat instead of live animals could be imported in steamships fitted with refrigerators. As a result of the depression there were few new developments in agriculture.

1900–1950

During the last 50 years there have been more changes in agriculture, in this country, than in the previous 2,000 years. These have arisen from applied research in agriculture and in other associated industries, following upon discoveries in the pure sciences and encouraged by threats of famine during two great wars.

At the turn of the century all transport on and off the farms was horse-drawn. Farm implements were horse-drawn, too. The farmer and his wife generally took the butter and eggs to market, once a week, in a dog-cart. Cattle and sheep were driven to market on the hoof. Pigs were carried in a horse-drawn float. In September flocks of 500 to 3,000 geese were brought over from Ireland and driven on foot round the country markets until they were all sold to farmers, who fattened them on their stubbles. Many of these geese walked 50 to 100 miles before being sold.

Crops of corn were cut with a mowing machine drawn by two horses, and five reapers could tie the sheaves as fast as the corn was cut. If the corn was lodged it was cut with a scythe or a sickle. But the sickle made the best job of it. The horse-drawn reaper and binder had, however, come to this country and was spreading. Machinery on the farm, for grinding corn, chaffing straw and rolling oats, was driven by a horse mill, or on some farms with a small steam engine. The steam plough, with two steam traction engines which drew a seven-furrow plough backwards and forwards across the field, had been in use for some years in a few districts where there were large farms.

Since those days 50 years ago there have been great changes. The farm tractor with its internal combustion engine has almost completely replaced the horse on the farm. It can plough from a half to 2 acres per hour, whilst the old horse plough generally did less than 1 acre per day. But it is not only a matter of saving labour. It enables us to get our land ready for sowing, during shorter periods of favourable weather. As a result of this we are able to sow our crops on an average perhaps 14 days earlier than we use to do ; at any rate, we do so in East Anglia. This means that we get heavier crops.

The combine harvester is rapidly replacing the tractor-drawn reaper and binder. So that now the large corn-growing farms, which used to need one man per 25 acres, now only require one man per 100 acres for the harvest, and they have threshed as well as reaped the corn. But with the combine harvester barns for the storage of the grain must be provided, and a grain drier is needed in some years. The use of implements manipulated by a hydraulic lift on the tractor enables a man

to perform from the seat of a tractor many of the tasks that used to be performed by hand. Farmyard manure can be loaded into spreaders by means of a loader in front of the tractor. A manure spreader, with power take-off from the tractor, will distribute the muck on the land. A buck-rake can be fixed to the hydraulic lift so that one man can pick up and carry to a silage pit from 10 to 20 tons of grass in a day. The multi-furrow plough attached to the hydraulic lift enables us to plough round and round the field instead of ploughing it into lands with ridges and furrows.

We know, from the appearance of plants, and from analyses of the soil, a good deal about which plant foods are deficient in the soil and we can correct these deficiencies by the application of nitrogen, phosphates, potash, lime, magnesium, boron, molybdenum, etc. Ample quantities of the nitrogen, phosphate and potash fertilisers are available. The nitrogen fertilisers are made in this country. Rock phosphate is imported and made into fertilisers. Potash, now imported, has been located in Yorkshire and will be mined in a few years.

We still use a rotation of crops to avoid the numerous pests and diseases which come from monocropping, and the hand hoe is still in use for weeding root crops. But we now have a number of selective weedkillers. We can kill charlock and other weeds in corn without damaging the crop. In some counties of England one can go for miles along the road without seeing a field of corn infested with charlock. Another weedkiller will destroy most of the weeds in a pea crop without doing any serious damage.

New insecticides are now available. They have made it possible to protect almost all crops from insects, e.g. they will completely control the flea beetle which often used to destroy our crops of cabbages and kale. They will protect our corn crops from wire-worm even on newly ploughed-up grassland. There are now sprays for fruit trees which protect them from almost every likely pest. We have seed dressings for corn and other seeds which protect them from fungus diseases and some insect pests.

We can buy farm seeds which can be relied upon to grow and to be true to type. We are also getting new and improved varieties of most farm crops, including the grasses. A technique of ploughing up old pastures and re-seeding them with suitable varieties of grass has been developed and used on both lowland and mountain pastures. There has been a return to the system of including three-year leys of grass in the arable rotation. The effective use of fertilisers on these temporary leys and on permanent pastures has been investigated and is now becoming established. The amount of food grown on pastures can be three to five times as much as it used to be under the old system. This development has not taken place as fast as we might have expected, but progress in the management of grass has been made, particularly by those preserving grass for winter feeding, either by grass drying or by making silage.

Arable silage has been made in this country since the 1880's but the process did not become at all general. In the 1920's and 30's methods for making silage from grass were worked out and they have come into general use on the large farms, but have not yet been generally adopted on the small farms. This is probably because the system of making it does not fit so well into the economy of the small farm. No doubt we shall find a way over this difficulty.

The drying of grass by artificial means was first tried in the 20's and has developed considerably. Dried grass is an important protein food and much of the Vitamin A for the winter feeding of our cattle, pigs and poultry is now provided in the form of beta-carotene in dried grass.

Experiments on the digestion of food by cattle led to a knowledge of the importance of proteins and carbohydrates in the foods, and, based upon this work, there has been developed a system of rationing which can be applied to cattle, pigs, sheep and poultry. From the analysis of a food and a knowledge of the digestibility of the constituents we know how it can be blended with other foods to make an economical ration for any class of stock. This knowledge has enabled us to feed our stock better than we used to do. It must have saved millions of tons of food from being used wastefully. It has been

the chief factor in increasing the yields of our dairy herds.

The discovery of vitamins has added to our knowledge of feeding animals. We can now prescribe for any particular class of animal—from a growing chicken to a dairy cow—the quantities of proteins and carbohydrates required, the specific proteins that must be present in the food, the quantities of seven or eight vitamins and the amounts of certain minerals such as lime, salt and phosphate which will be needed. Some of the vitamins are easily provided as they occur in the foods we have on the farm. Others which might be more difficult to obtain can now be bought in a concentrated form for inclusion in food.

As we are able to provide a complete food we are able to employ intensive and labour-saving systems of pig and poultry management.

During the last 20 years there have been great advances in the science and practice of medicine. These have been followed by similar advances in the science and practice of veterinary medicine. Not many years ago when we called in the veterinary surgeon to a sick animal he either gave it a drench or recommended that it be slaughtered. Now we have a scientific diagnosis—often made by the Ministry of Agriculture Veterinary Laboratory—before treatment, which is on the same lines as the treatment of human ills. New drugs, sera and antibiotics have been manufactured for veterinary use. The developments in veterinary technique have enabled us to avoid losses and have made animal husbandry, in all its branches, more efficient.

The dairying industry has seen great changes. The Hosier bail and the milking parlour are taking the place of the old cow houses. The milking machine is replacing hand milking everywhere. Steam or chemical sterilisers are used daily for all the utensils in the dairy. Clean milk only is produced. Mastitis has been eliminated from many herds. The average yield of milk per lactation has been increased by perhaps 200 gallons. Artificial insemination, which has been introduced recently, is convenient and economical, especially for small dairy herds. If we know enough about breeding and if we use the knowledge, it will improve all our herds. Even if we do not know enough it will be no worse than the present system. One or two of the breed societies are attacking the breeding problem seriously.

Many of the factors which cause losses of sheep on mountain pastures have been determined and are being eliminated.

Pig husbandry has been improved by better housing based upon more exact knowledge of the pig's requirements, by the use of warmed creeps for piglets and by better feeding. These can increase the number of pigs reared per litter by perhaps 10 per cent.

New and cheaper methods of rearing chickens have come into use. We can keep hens healthy and producing eggs all the winter in deep litter houses or in batteries, with great economy of labour.

As an indication of the increased efficiency of farming in this country during the last 50 years we can compare the average yields of wheat per acre. In the open fields the average production of wheat was probably 4½ cwt. (8 bushels) per acre. When the land was enclosed there was a general increase to about 9 cwt. (16 bushels). The Hertfordshire farmers between 1750 and 1800 were producing 13 cwt. (24 bushels). The yield in England and Wales from 1934 to 1938 was 18½ cwt. (33 bushels) ; in 1951 it was 22 cwt. (39 bushels). But this is not the end of the story. I have produced 36 cwt. (64 bushels) per acre for the last 9 years by using a lucerne ley in the rotation. Many other farmers are also getting similar high yields. I do not know what is going to be the limit. I have heard of freak crops of 55 cwt. (100 bushels).

The agricultural industry comes very much under government control. We are set a target for expansion (e.g. 60 per cent. above 1939 production). When we get near the target, prices are regulated to prevent us exceeding the target. More milk is wanted in the winter so higher prices are paid for winter milk. We respond quickly and produce more milk in winter than is needed, so prices are lowered to step down the production. Agriculture is becoming so efficient that it responds too quickly to even small money incentives. But these changing demands

are very disconcerting to the farmer. I hope we shall find systems of control which cause less disturbance to the industry than the methods which are being used at present.

The government control of production is not new. It has been going on for hundreds of years, and some of these controls have worked well. An Act of William and Mary passed in 1689 regulated the price of wheat for over a hundred years. Under this Act a bounty of 5*s*. per quarter was paid on any wheat which was exported, when the price of wheat in this country dropped below 48*s*. per quarter. Although the population of the country was doubled during the century the price of wheat was usually between 40*s*. and 50*s*. per quarter. In only three years did it rise above 60*s*. and it never fell below 30*s*. These were small variations compared with the price changes in our time. During the three years 1936 to 1938 I sold malting barley at one time for 30*s*. per quarter and at another for 60*s*. per quarter.

In 1689 the object of the government was to encourage the production of a steady supply of wheat for the population. For if the harvest was too small the country starved. If the harvest was too large, the price fell so low that insufficient wheat was grown in the following years. They succeeded in diminishing these fluctuations.

Our governments today have not so simple a problem. Before the first world war there was such a demand, in other countries, for our manufactures that we had to leave much of the payment we received for our goods, as investments in those countries. The dividends from these investments were usually sent here in the form of food, raw materials or gold. As gold was not always available we had to accept raw materials and food. As we had always a buyers' market for food, it was very cheap, so we produced less and less food on our farms. The idea developed that our industries depended upon cheap food to keep down the cost of production of our exports.

Now the outlook is somewhat different. We have spent most of our investments abroad in paying for two world wars. We must ceaselessly export our manufactures to obtain raw materials, luxuries and any food we require above what we produce at home. If we can keep our exporting industries in full employment we can do this and our people may enjoy a standard of living higher than that existing in most of the countries to whom we export our manufactures. If we cannot keep the cost of our exports low enough to maintain our markets, factories will be shut down and there will be unemployment in industry generally. So it is argued that we must maintain our exporting industries in full production by partially closing down our agricultural industry again and buying cheap food from countries with a lower standard of living than ourselves.

We have already been told that we are not required to increase production above its present level. It may be necessary to keep it at this level, so that we can be ready to come into full productivity in time of war. But we hope the chances of war will diminish. Shall we then be asked to reduce the present volume of production?

For nine years since the end of the war we have been controlled. We have received subsidies on some products to lower the cost to the consumer, but we have sold other products, e.g. corn, below world market prices. The control of the industry was designed for war famine conditions and it has lasted until this year.

Other industries were decontrolled soon after the war. They were able to reorganise for peace-time production when other nations were unable to flood the market with their goods.

If agriculture had been freed immediately after the war and given the same facilities as other industries, this country would have had to pay world prices for its food just as it did for clothes and for all other products.

Owing to control the methods of marketing our produce have remained completely stagnant. We have neither the organisation nor the physical assets to sell our produce in competition with well-organised imports. Organisations are being improvised as rapidly as possible, but we sadly lack well-equipped abattoirs, cold-storage plants, bacon factories and milk conversion factories.

The annual gross value of our agricultural produce is now about one thousand million pounds. It is still about our largest industry and it provides a market for the manufacturing industries which is greater than most of their foreign markets.

It would be interesting to know what proportion of the output of various industries is absorbed by our agricultural industry and its workers.

There are several industries which have grown directly from scientific research during the last 80 years. They inherited very little from the craftsman. Examples are the electrical engineering industries and the modern chemical industry.

Other industries, based upon an ancient craft, such as the textile industry, had settled down to methods of manufacture hundreds of years ago. The industrial revolution speeded up their machines and drove them with water power or steam engines instead of by hand. It moved the work from the cottage to the factory but it did not alter the process very much.

Agriculture, an ancient craft, started to approach science for help 150 years ago. When, about 1790, the government became concerned about the food supply for an increasing population, the Board of Agriculture was formed and a few years later Humphry Davy was appointed Professor of Agriculture to the Board. He gave a series of lectures on agriculture but he was not able to suggest any great changes—we had to wait nearly another hundred years for these. However, a start was made about 1840 when Lawes at Rothamsted began to make systematic experiments with fertilisers and Liebig endeavoured to make some progress in agricultural chemistry.

During the industrial expansion of the nineteenth century pure science made great advances but very few scientists applied themselves to the old-established industries. At that time the only respectable employments for a scientist were research and teaching.

It was not until the second year of the first world war that we began to say ' Industry ought to make use of science.' Then within two or three years the Department of Scientific and Industrial Research was formed to organise Research Associations in industry. Research Associations have now been formed for most industries—but not for agriculture. For the government had already started financing some agricultural research at that time and the volume of research has been continually expanding for 40 years. It now covers most of the sciences that can be applied to agriculture. This is the work of the Agricultural Research Council under the Lord President of the Council. Under the Ministry of Agriculture there is the Agricultural Improvement Council with its Experimental Husbandry Farms. Then we have the Agricultural Advisory Service with an organisation in every county, and district officers covering every farm. The county organisations are grouped under provincial headquarters where there are laboratories and specialists in the different branches of agriculture.

Research in pure science and in its application to agriculture is also carried out by manufacturers who sell their products to farmers. The manufacturers seem to be able to apply the successes in their research laboratories quickly to the industry. Their organisation is flexible. Some of their research workers can go out of the laboratory on to the development stage of the work, for a time, and return to their laboratories with experience and new ideas. A research worker in any industrial organisation may start a new line which promises to make an improvement in agriculture and lead to a new industrial development. It must not be supposed that research in the laboratories of manufacturers must always have an immediate sales objective in view. The hormone weedkillers arose from an experiment with naphthyl acetic acid (which was active as a plant hormone) to see if it would kill plants and, if so, if it was selective in its action on different plants. It was found to be selective. But it was too expensive for general use. A search was then made for other compounds—some of which might be cheaper. Other active compounds were found. They were not much cheaper per pound, but 2 lb. per acre was as effective as 40 lb. of naphthyl acetic acid, so they were cheap enough for use on farm crops. They were tried out first at an experimental farm and then demonstrated on ordinary farms all over

the country. They received considerable assistance at this stage from Professor Blackman and his team working for the A.R.C. Then men trained in science and in agriculture found out as much as they could about the product. These men assisted the salesmen in selling to farmers and to agricultural contractors. Instructions for killing numerous weeds are printed and distributed to customers and prospective customers. The manufacturers' advisory officers call and advise any farmers who have a special problem. In a few years a series of differently selective weedkillers were developed and their use has become general all over the world.

The introduction of a new craft or a new technique into any industry is never very easy. Let us consider a few of the improvements of the last 50 years, which I have mentioned, and see how the farmers were induced to adopt new practices.

Feedingstuffs.—The present system of evaluating foodstuffs, from a knowledge of their analyses and from the digestibility of the proteins and carbohydrates in the food, arose from the scientific investigations of O. Kellner in Germany. But the system was not developed there. Possibly because their small farms were mostly self-supporting in feedingstuffs, accompanied by a rather low standard of animal husbandry. But in this country it was taken up by Woodman and later by Watson and others. Woodman wrote a bulletin on the subject for publication by the Ministry of Agriculture. Now long before Kellner's work linseed cake and groundnut cake, which have a high protein content, were sold in this country for feeding to calves and to cows in milk. The information in Woodman's bulletin enabled the firms selling these oilseed cakes to make up a better balanced food for dairy cows by blending them with cereals. They soon made this food into cubes which looked something like the oilseed cake which farmers had been accustomed to buy. A Ministry of Agriculture committee reported on the feeding of dairy cows and the report helped further to establish the system of feeding compound feedingstuffs. The agricultural colleges and, especially, Professor Boutflour taught farmers how to feed their cattle with these compound foods. They helped the salesman who sold to the farmer.

Vitamins.—After the discovery of Vitamins A and D in the 1920's, cod-liver oil gradually began to be added to pig and poultry foods. Later, sun-dried lucerne meal was imported to provide Vitamin A and this was later replaced by artificially dried grass and lucerne which were much richer in the vitamin. The use of these and other vitamins has become general since the publication of the report of a committee in U.S.A., which estimated the minimum requirements of each known vitamin for different kinds of stock.

We know approximately the quantities of some fourteen different vitamins and trace elements required for the different classes of livestock. We know which of these are naturally present in sufficient quantities in the food. And we know what vitamins we must add. We can buy these at a chemist's shop. But there are not yet many farmers who make up all their own pig and poultry foods. When they have acquired the technique they will be able to make use of foods produced on the farm without selling them and buying them back again from the mixers.

The compounders sell foods containing the necessary vitamins but they are not always sold with a guarantee of the vitamin contents, nor do they always specify what vitamins have been added.

Fertilisers.—The work at Rothamsted, extending over 50 years, had demonstrated the importance of nitrogen for farm crops. It was then available in the form of Chile nitrate and sulphate of ammonia, the latter being a by-product of coke manufacture. There was no other source of nitrogen in sight, but it was known that, in an electric arc, the nitrogen and oxygen in the air could combine to form nitric acid. Then in 1898 Sir William Crookes, in his Presidential Address to this Association, pointed out that the population of the world was increasing rapidly and he foresaw a world famine in a few years unless we found a method of fixing the nitrogen of the air to make fertilisers. Three methods of fixing nitrogen were developed in the next dozen years. The synthesis of ammonia was eventually found to be the most practicable and is now a very large industry.

The fertilisers were sold to farmers by the manufacturers, who carried out experiments on their own experimental farms and gave demonstrations on customers' farms. The agricultural colleges also helped, but the chief sales effort came from agricultural advisors who assisted the salesmen to sell the product. These agricultural advisers were usually employed by the manufacturers.

Silage.—The method now used for making grass silage in this country was worked out by Watson at Jealotts Hill as part of the policy of investigating methods of using the large quantities of grass which could be grown by fertilising our grassland with nitrogen. It was introduced to the farmers by the fertiliser manufacturers and during the war by a campaign organised by the Ministry of Agriculture. Rex Paterson has imported and developed a silage process which is used in Canada and U.S.A., and he has obtained very good results. O'Neal in Northern Ireland has obtained spectacular results with Watson's process.

The making of silage is a process which should be economical. Farmers in general, and particularly the small farmers, do not, however, seem to be able to fit it easily into their economy. We shall have to get modifications of the silage process tried out on farms of various sizes and see how these modifications work out in actual practice. If some mechanical equipment which really worked had been needed to make silage, the manufacturers of the equipment would have demonstrated how to use it, sold their machines and established silage-making in the country. But as no one has anything valuable to sell, the process is not yet perfected. Usually the publication of the results of experiments is a first step which must be followed up with farm trials under varying conditions until the process is an economic success on ordinary farms.

Grass Drying.—The drying of grass, suggested by Woodman, was developed by the manufacturers of nitrogen fertilisers. With little or no encouragement from the Ministry, as much food is now preserved as dried grass as by silage. I believe this is because the manufacturers have been able to sell really effective drying plants to farmers.

Dairy Industry.—The developments in the dairy industry have been spread by the Milk Marketing Board and particularly by the salesmen of various machines and equipment. The scientific basis for these improvements came almost entirely from the National Institute of Dairy Husbandry.

Artificial Insemination, originated in research laboratories, has been made generally available by the Milk Marketing Board, who have established and run centres over most of the country. There are also local independent centres.

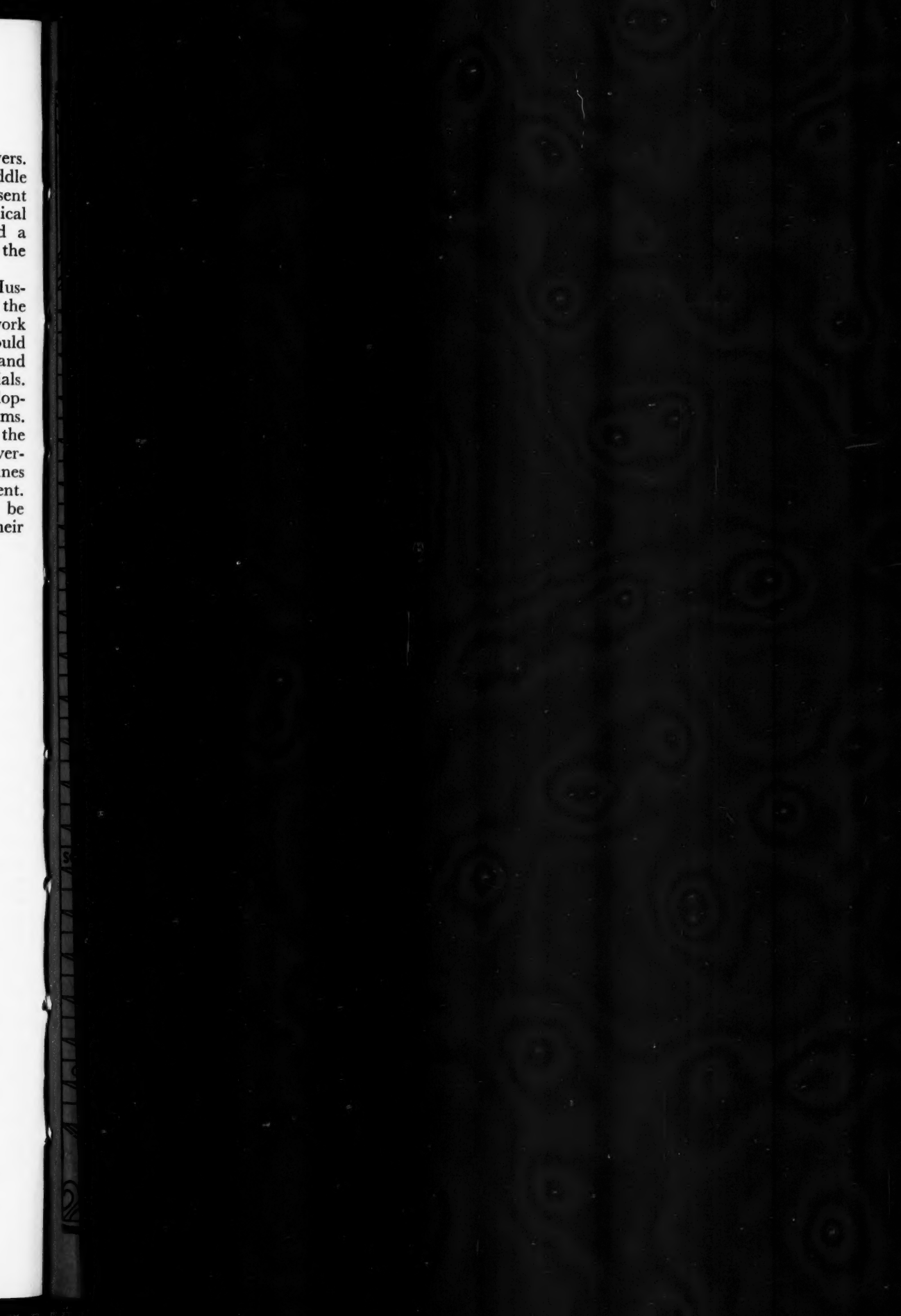
Insecticides.—The insecticides BHC and DDT were evolved from long and tedious work and much perseverance in the research laboratories of the manufacturers. This was *ad hoc* research. A synthetic insecticide was needed because the vegetable insecticides pyrethum and derris were never obtainable in sufficient quantities. These new insecticides were better than we had ever hoped for. There was already a demand for them and in about 10 years an enormous industry has grown up. They have made it possible to prevent the migration of locusts across a continent.

It appears that, with few exceptions, scientific developments in agriculture are most quickly introduced to the farmer by well-informed salesmen or an organisation such as the Milk Marketing Board.

In some other industries the Research Associations introduce new scientific developments quickly into the industry. They may have a development or service department which advises and helps individual firms. In this way they show the manufacturer how he can make his production more efficient. They may devise a scientific instrument perhaps for measuring very accurately the changes in the tension of a thread in a rapidly moving machine and with this they can analyse the causes of faults in the product. They get a machine and work it themselves until it is perfected. They then fit the arrangement on to a machine in one or more factories where it is used for some months. When it is running consistently it is offered to every manufacturer—who may see it working either at the Research Association laboratory or at a factory. Some quite small Research Associations which have been in existence only a few years have already introduced important developments into their industry.

There are thirty or forty Agricultural Research Institutes and Units financed by the State and supervised by the Agricultural Research Council, which co-ordinates the work of the scientists. They spend about £3,000,000 per annum. This is quite a moderate expenditure for an industry with an annual turnover of £1,000,000,000.

The Agricultural Improvement Council, under another minister, has now seven Experimental Husbandry Farms, and I understand the number will be increased. They are working to solve farm problems. Also in the Ministry of Agriculture is the Agricultural Advisory Service in touch with the farmers. There are thus three layers, each covering all agriculture, (1) research, (2) experimental husbandry and (3) advisory service. Each layer is in itself doing very fine work and is excellently co-ordinated. But there is no vertical drive through the three layers. Except perhaps a restlessness in the middle layer. Perhaps some day the present organisation will have added to it vertical as well as horizontal direction, and a freer interchange of personnel between the different layers.

An officer at an Experimental Husbandry Farm might be in charge of the conversion of a successful piece of work into agricultural practice. He would arrange for trials on commercial farms and interest the advisory service in these trials. He would have to follow up the development on to small as well as large farms. Great achievements are always due to the drive and persistence of men who overcome all difficulties and stake their fortunes or their reputations on the achievement. The men are there. I hope they will be given the opportunities to show their worth.

Printed in Great Britain
SPOTTISWOODE, BALLANTYNE & CO. LTD.
London & Colchester